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PART I.

ORIGINAL COMMUNICATIONS.

ART. I.—*Some Remarks on Gonorrhea and Syphilis.* By MAURICE HENRY COLLIS, M.B., F.R.C.S.I., Surgeon to the Meath Hospital and County Dublin Infirmary; Member of Council R.C.S.I., and of Council of Surgical Society.

ALTHOUGH few persons will now pretend to add much to the symptomatology or treatment of gonorrhea and syphilis, yet all will allow that there is still room for the honest records of experience in both departments. As I have ventured to show, in the pages of this Journal, there is no domain of surgery in which dogmatic assertion is so prevalent a feature; and where dogmatism is found to exist, much imperfection of observation and uncertainty of action may fairly be suspected. Such must be my apology for writing on a subject so hackneyed, and of which most practitioners will think they have already had more than enough. I do not now presume to do more than state the results of a very moderate experience in the treatment of uncomplicated forms of gonorrhea and syphilis, founded not alone upon my own practice, but upon such observations as I have been able to make of the practice of others.

Dr. T. Chambers asserts that gonorrhea, if let alone, will get well spontaneously in two or three weeks. Certainly the milder forms get well on surprisingly little treatment. For years I have not given either cubebs or copaiva at all. They are, perhaps, useful drugs, but I have not found it necessary to use them; the disease, as I have met with it, admitting of a ready cure by less unpleasant remedies. In persons of full habit and great vigour I generally give a saline purgative at the commencement, followed up by minute doses of tartar emetic, if there be much constitutional disturbance or a tendency to high local inflammation. Cold affusion will also be of service in these cases. The main treatment, however, is by injections; not heroic solutions of nitrate of silver, which are eminently uncertain and dangerous in their action, but by weak, and frequently repeated, solutions of alum. The rule for applying injections is simple:—If the inflammation be severe, let the solution be weak and frequently used; if it be of chronic type, let the solution be strong and seldom used. This is the key to the successful use of injections; and not only is the rule applicable in gonorrhea of the urethra and of the conjunctiva, but it may be extended (with necessary modification) to treatment of all inflammations, and to the use of internal remedies as well as external applications. Indeed, I think, a similar rule may be traced in the effects of all stimulating applications, whether general or special, in the use of tonics, astringents, and sedatives; so much so as to convey to my mind the existence of a general law regulating the actions of a large class of remedial bodies. The terms of it would run something in this form:—"The more acute the disease, the more frequent and the weaker the remedy; the more chronic, the stronger and less frequently applied."

The astringent on which I chiefly rely for the cure of gonorrhea—is alum. In the most acute form of gonorrhea, when the discharge is profuse, thick, and glutinous—the lips of the urethra red, villous, and pouting—the patient should be directed to pour a small jug of cold water over the organ, and immediately inject a syringeful of solution of alum of the strength of half a grain to the ounce. This injection is to be repeated every half hour for the first day, and as often at night as the intervals of sleep will allow. In all probability, before 24 hours have elapsed, the secretion will be lessened in quantity, and somewhat thinner; the local heat, swelling, and redness will have abated; and the ardor urinæ will have almost disappeared, if the saline purgative and the tartar emetic have been

used along with the local treatment. The injection may then be increased to a grain to the ounce, and used every hour; in all probability, after the lapse of 48 hours more, the discharge will have ceased entirely; the injections, however, must be continued for another week or two—but at the strength of half a drachm to the eight ounces—three times a day; otherwise a relapse may occur, which will be harder to cure than the original clap.

Four objections are made to this plan of treatment:—The difficulty of carrying it out in its integrity; the irritation produced at the orifice of the urethra by the repeated use of the syringe; the liability to cystitis, by the gonorrheal fluid being carried into the bladder, and the inefficiency of such weak solutions.

To the last objection I can only oppose my experience, backed by the evidence of such surgeons and pupils as may have observed my plan of treatment at the Meath Hospital for the last eight or ten years. Besides, reliance is not exclusively placed on the injections; the lowering effects of the purgative and tartar emetic must be taken into account.

Cystitis is not likely to occur, because the proportion of alum is quite sufficient to destroy the infectious character of the pus by coagulating it, and to leave a residue for astringent action on the urethra besides. Injections of plain cold water might otherwise be used at the outset, in place of the alum, but for this risk of cystitis. It has, however, happened, that such injections have set up inflammation of the bladder, by sending on an unaltered pus into that organ, and, consequently, I have always added to the water a sufficiency of alum to prevent that accident.

Irritation of the orifice of the urethra, in consequence of the frequent use of the syringe, is to be avoided by having a very smooth syringe of glass; by only introducing it so far as is absolutely necessary, and not letting the point enter as high as the ridge corresponding to the corona glandis; and by douching with cold water before and after syringing.

Lastly, there is, no doubt, difficulty in having these directions fairly carried out, for obvious reasons; but the case I have supposed is an extreme one, in which some degree of confinement will be unavoidable, and, consequently, this difficulty will be more easily met.

In the commoner forms of gonorrhea, where local inflammation and general fever are not severe, and where the discharge and ardor urinæ are the only sources of discomfort, little internal

medicine need be given—a purgative pill or so if necessary, and a few drops of liquor kali in gum-water or camphor mixture, with a pill of hyoscyamus, camphor, and morphia at night, will be enough. Here, however, the judgment of the surgeon will be called into play in each case to apportion the strength and frequency of the injection. The general rule has been given above; in carrying it out, the amount of discharge—its condition, purulent or half mucous—the appearance of the orifice of the urethra—and the amount of ardor urinæ will guide us. If the discharge is still abundant, puriform, and accompanied by redness of the lips of the urethra, and by considerable scalding, we must use weak solutions (eight to twelve grains to the eight ounces of water), every second, third, or fourth hour; but if, on the other hand, as will occur in old stagers, the discharge is half mucous, the lips blue and the scalding not complained of much, we may advance in strength to a drachm, and diminish in frequency to three or four times a day. If a syringeful is injected we may spare our patient much of the unpleasant scalding by injecting before micturition.

This treatment will cure an ordinary gonorrhea in from four days to a week—at least, it will in that time stop all discharge; but, as in the acuter types, the injections must be continued for several days lest a relapse occur, as is sure to be the case after excitement or errors in diet, and sometimes without such provocation.

Gleet, the opprobrium of this branch of surgery, is not unfrequently kept up by over anxiety to cure it. The introduction of a bougie smeared with lard and dusted over with powdered alum, or a single touch of nitrate of silver by means of a *porte caustique*, not repeated more than twice, and at an interval of a week, will sometimes succeed, provided nothing else is done in the interval, and the organ get sufficient rest. I have occasionally cured a gleet by first administering muriated tincture of iron and tincture of opium, in doses of fifteen drops of the former to five of the latter, three times a day, and thus rendering the discharge more genuinely puriform. When this effect follows, a spontaneous cure will sometimes arise; and if not, the injections of alum will often now succeed, though they previously failed.

The objections made, with some justice, to the use of strong solutions of nitrate of silver, do not apply to alum. Stricture, inflammations of the prostate or bladder, are manifestly unlikely to arise from the plan of injection which I have found sufficient;

both the material and strength at which it is used are much milder than even a very moderate solution of lunar caustic; and even at full saturation, or when applied in substance, alum is a simple astringent, reducing the calibre of vessels and contracting redundant tissue to natural dimensions, not destroying it in any degree, as nitrate of silver does, nor producing a cicatrix with the contractile tendencies of a burn.

Gonorrheal Ophthalmia.—This affection—so formidable to the surgeon to deal with, and so fatal to the usefulness of the eye—yields with marvellous rapidity to repeated weak injections. The inflamed and œdematous conjunctiva being punctured, or snipped with the scissors if necessary, a careful student can be put beside the patient's bed, and shown how to send the contents of the syringe underneath the upper lid, from the external canthus across the eyeball. In the most acute cases a solution of a quarter of a grain of nitrate of silver to the ounce of distilled water should be used every ten minutes, for the first hour; after that a half-grain solution should be injected every half hour. If this is carefully carried out for the first twenty-four hours, the patient's eye will be quite safe. A stronger solution may then be used; and, if needful, it may be followed, in a couple of days, by Guthrie's ointment of nitrate of silver, if the villous condition of the conjunctiva should seem to require it. I have followed this plan of treatment generally, for at least nine years; and in that time I have never lost an eye from gonorrheal ophthalmia, with one exception: in that case the pupil in charge broke the syringe, and, thinking it a matter of no importance, he waited for twenty-four hours to get it replaced; by this time the cornea had sloughed in one point, and the iris protruded. The man, however, was so fortunate as to recover, with comparatively slight injury to sight. Such surgeons and pupils as followed any of these cases have been struck with astonishment at the facility with which this formidable affection is cured. I cannot at this moment remember to whom the credit of weak injections of nitrate of silver is due; my attention was drawn to it by seeing the failure of the heroic treatment, which sacrifices nearly 50 per cent. of eyes in whole or in part. I found, however, that the weak solutions were insufficient for the cure of the disease unless frequently applied.

Syphilis.—The observations of most surgeons will confirm Ricord's doctrines on the variations of syphilis, as modified in his later years, and rendered familiar to us in the writings of Lee and others. I use the term variations as a better one than varieties, because it

does not dogmatically assert a variety of poisons. This necessity for a variety of poisons, instead of a variation in the poisoned, seems to me the weak point of the Ricordian theory. Indeed, Ricord himself recognizes the individual as the modifying agent of the infection; and we may fairly give our consent to the view that both the source of the poison (an individual) and the place of deposit of the poison (an individual) impart peculiarities or individualities to the poison in each case. Accepting this view, and bearing it in mind, it matters less whether, for convenience of memory, we use Ricord's, or Lee's, or any other man's grouping of individualities into classes. Carmichael's groups were too fanciful, ran too much to an extreme fineness for truth; so also are, and must be, all systems. They are generally true, critically false, and much of the virulence of opposing systematizers is due to forgetfulness of this fact. Such being my creed, and reared, as I was, in the practice of a school which considered mercury as a *sine quâ non* in all cases of syphilis, I may, perhaps, incline too much to the opposite conclusion, that syphilis and mercury not only have been, but still are, too much regarded as opposing forces—that mercury is not a specific for syphilis in any proper sense of the term, and that, in the large proportion of cases of syphilis, no mercury need be given whatever; while in the remainder it is to be given on the same principles and in similar doses as form our practice in other diseases. I do not believe mercury to be required at all in soft chancre, as a rule, unless the use of black wash, to heal the local sore, deserve the name. Soft chancre is so seldom accompanied by any constitutional disturbance that it requires little internal medicine; a blue pill may be needed once in a way, as in any complaint, to regulate the liver. When secondaries follow soft sores, they are so mild as to get well with very slight measures; and I am quite sure I have seen mercury, even in what is called moderate doses, do harm in such cases. Tepid salt-water baths, and small doses of the iodides of potash, and, if necessary, of iron, a little bark, some chlorate of potash, cod-liver oil; or, if a case be at all obstinate, a mixture of iodine, iodide of potash, and arsenic, after the formula given by Neligan,[a] have

[a] R Iod. potassii gr. xvi.
Idinii puri gr. iv.
Liq. arsenicalis sc. iv.
Syrupi aurantii flor. oz. ii.—Misce.

Every drachm contains five minims of arsenical solution, one grain of the iodide, and a quarter of a grain of iodine. I have found this combination most successful in obstinate psoriasis and pityriasis. M. H. C.

succeeded in the few cases of secondary roseola or pityriasis which I have met with after soft chancre. Cleanliness and simple lotions will cure the local sores. Mischief invariably arises in these cases from allowing the purulent discharge to accumulate, become acrid by decomposition, and produce excoriation; frequent washing with water, and slightly astringent lotions, will effect a rapid cure.

Of the local complications which arise with soft chancres there is no need to write. Phymosis, bubo, warts, &c., are subjects on the treatment of which no very material difference of opinion exists; buboes, indeed, may be opened by a puncture, if they be seen when the skin is still sound; but when it is undermined, a free incision will probably be needed; but of this every surgeon will readily judge in each case for himself.

It is essential, for the proper and scientific treatment of any disease by a given medicine, that he who uses it should thoroughly understand its mode or modes of action. Does mercury act on constitutional syphilis chemically, destroying a poison which circulates in the blood, or which is deposited through the tissues? In other words, is it an antidote? or does it cause the elimination of such a poison through its stimulating effects upon eliminating organs? Or again, does it so improve the organs of nutrition that they supply a healthy pabulum which shall displace what is diseased when this latter is worn out? Or, lastly, does it cause the death of tissues enfeebled from the moment of their organization by the debilitating effects of an animal poison? And, as regards itself, is it eliminated along with the dead tissues, or does it remain in the system to influence subsequent nutrition for good or evil? Some pathologists may hold to one view, some to another. For myself I must confess I do not believe mercury to be a specific or an antidote, in any sense, to syphilis. I do not believe it improves the assimilating powers; I do not believe it has the power of eliminating the poison through natural organs; but I do believe that it acts as a poison on feeble and ill-organized material; and that if not itself expelled from the system, it will exercise an influence for evil upon subsequent nutrition, proportioned to the amount that has been introduced into the system, and the feebleness of the individual. Doubtless we must be satisfied to argue a good deal from analogy as to its mode of action; but circumstances enable us to do so with almost absolute accuracy. We are fully acquainted with its anti-phlogistic powers, and we can almost see its mode of action in one form of venereal disease, so that it is not so presump-

tuous to conclude upon its general mode of action as might at first sight be supposed. In venereal iritis, by administering it in such a way as to cause its rapid absorption, we check the organization, as the phrase goes—that is to say, we cause the death of certain lymph which is effused from the inflamed and congested vessels of the iris, and so we save the pupil from being blocked up, the iris from contracting adhesions to the capsule of the lens, and from becoming permanently thickened and sluggish in its action. Here we have the power of seeing, in one case, what happens if the disease be allowed to run on unchecked, and in the other we can see with equal ease the effects of the mercury at all stages of its exhibition. Following, by analogy, its apparently similar action in peritonitis and other acute inflammations, we are justified in supposing that in these also it kills the lymph; we have other means of knowing that such lymph is degenerate, and capable only of imperfect organization. Again, we find that mercury, when administered in a suitable manner, will cause the absorption of certain deposits of lymph already more or less organized, as in the hardnesses of many chronic inflammations. Are we not justified in concluding that it causes their removal simply by depriving them of life, and leaving them free for the ordinary processes of removal? Finally, we find it exercise an active influence in the removal of the plastic effusion which forms the essential feature of the hardened chancre and knotted bubo; and are we not justified by all analogy in concluding that the principle of its action is identical, in this case also, with what it is clearly proved to be in the other—that it kills the morbid lymph where deposited in the tissues? Can we go a step further and say if it exercises any similar power over lymph which still circulates in the fluid state? Here direct facts fail us; but if we look at the pale cheek of one who has been salivated, if we feel his wasted and flabby muscles, and note the slow beat of his weakened heart, we have good reason to conclude that the mercury has killed more than the deposited lymph, and more than such lymph of feeble vitality as it found circulating in his vessels. Is it not, then, true, that in handling mercury we handle a two-edged sword, and that to wield it wantonly is to kill, and not to cure. This much may certainly be affirmed—that every grain of mercury which is exhibited will kill its proportion of living tissue; and if there be no tissue enfeebled by disease, the energy of the poison must be spent on the destruction of the sound. Let no man say that a few grains of mercury too much are neither here nor there. The worst case of

salivation I ever saw, short of death, was produced by six grains of grey powder, given in doses of two grains, on alternate nights, to a full-grown man. It was profuse and lasting, and brought the patient to death's door. No doubt he was a wretched strumous creature; and the surgeon was not to blame for the idiosyncracy which produced such unpleasant and dangerous results. The medicine was given as an alterative for strumous ophthalmia; but the case shows that a small dose of the mineral acts as a poison, and that the amount of mischief will depend upon the individual. To come, however, to the application of principles, I may fairly be asked, how is a hard chancre to be treated? and to this I reply, many have treated them not without mercury altogether, but with mercury where needed, and in conjunction always with remedies which will tend to obviate the almost unavoidable injury which mercury, however carefully given, will do to the healthy tissues of the body. Such men tell us—and we have no right to question their truthfulness—that other medicines will produce the elimination of syphilized structure. Thus, Weedon Cooke gives instances in which hard chancre have been cured without a grain of mercury, the hardness disappearing, and no secondaries following.

For my own part, I must say that genuine hard chancres, as described by Lee, are comparatively rare; but in many cases a hardened base exists, which is very easy of resolution after the sore has been healed by black wash; for this purpose frictions with a weak ioduret of mercury ointment will suffice, or with mercurial ointment itself. Such forms of chancre are found most often in those of sallow complexion and greasy skin; and for them a few grains of blue pill (two or three grains) twice in the week, will be serviceable. The genuine hard chancre, with a base that is likened, by Ricord, to a piece of parchment under the skin, is very rare, and requires more mercury, or rather it requires a full dose (five grains of blue or Plummer's pill) twice a week, for two, three, or four weeks. In both cases the mercury may be continued until it begins to resolve the hardness, provided we see no signs of its acting as a poison. Should it produce pains in the bowels and diarrhea, a quickened pulse, and loaded tongue, with symptoms of malaise, not to say febrile exacerbations, it must be laid aside—at least it ought to be. In any case, as a rule, I suspend the mercury the moment an impression has been made upon the hardness, and I allow the medicine which has already been taken to expend its energies completely before giving another dose. As long as the process of

resolution goes on, further doses are not needed; but when it ceases they can be resumed. In this way I am careful not to give a grain of the medicine needlessly, and the patient's strength is husbanded. Rapid administration of mercury, in the ordinary method of three doses per diem, or even one dose every night, induces a febrile condition, with disturbance of the digestive powers; and it is well that such a result occurs, for by this means intestinal irritation is set up, and a large portion of the poison is got rid of in the excretions of the bowels, otherwise we should often witness the evil effects of the drug in breaking down the strength of patients. The medicines to which I chiefly trust as co-agents with mercury, in obtaining the removal of syphilized tissue, are, 1st, those which gently stimulate the skin, liver, and kidney; 2nd, the whole range of tonics, vegetable and mineral, with cod-liver oil, and generous diet, in debilitated or strumous habits. The iodides of potash and soda will often suffice to complete the work of absorption commenced by mercury; they are milder, though very similar, in their action, but are by no means so harmless as to be suitable for indiscriminate use. In those who have been previously over-dosed with mercury they are useful, from their attraction to the periosteal tissues, in which mercury seems to have a special aptitude for depositing itself; but, as the object of this paper is rather confined to the consideration of the more common and uncomplicated forms of venereal diseases, and to express my conviction that their cure is neither mysterious nor difficult, I shall not enter upon any of the more abstruse points connected with either syphilis or gonorrhea. It will be enough to add, that in many cases where mercury has been abused, the organs of elimination are so deadened as to answer only to further doses of the mineral—all milder stimuli being ineffectual in stimulating their exhausted energies. In aggravated secondaries arsenic exerts a powerful influence, in combination with iodine and mercury.

In one form only of the disease—viz., the congenital—there seems to be no medicine but mercury capable of effecting a cure; this is plainly owing to the almost universal degradation of the tissues of the body. In such cases, as long as the medicine is required it is borne in larger doses, proportionately, than in any form of the disease in the adult. Where the appetite of the little patient has remained good, and its powers of assimilation unimpaired, a rapid formation of healthy structure fills up the place of the morbid, which is removed by the aid of mercury; but where the appetite fails, and cannot be restored by mild stimulants, the case is hopeless, the little

one had better be let die in peace, than have its agonies increased by the superaddition of what will then become an irritant poison. In the hopeful cases of congenital syphilis, mercurial inunction by Brodie's stocking seems the mildest and most effectual mode of treatment. In suitable cases I have never seen it fail, either alone or with small doses of grey powder every night. Assimilation and elimination are rapid processes with the child; and to this is due the comparative facility of administering mercury beneficially in congenital syphilis.

Art. II.—*A Statistical Inquiry into the Prevalence of Numerous Conditions affecting the Constitution, in* 1,000 *Phthisical Persons, when in health.* By Edward Smith, M.D., LL.B., F.R.S., Assist.-Physician to the Hospital for Consumption and Diseases of the Chest, Brompton, &c.

The following inquiry has occupied a large part of seven years, and is intended to show, at one view, the prevalence of a large number of conditions which are believed to modify the constitution, or which are evidences of modified constitutions in consumptive families. It is, therefore, primarily, an investigation limited to a particular class of persons—a class the largest which could be selected upon the basis of disease; but I have thought that whilst such information could not but be of value to life assurance offices and others in reference to this class, it would form a point of comparison with which similar inquiries in other classes, and indeed of the general community, might be compared, if any inquirer should think proper to extend it. The extent is much greater than any heretofore recorded, and, so far as I am aware, its aim has not been previously anticipated.

The conditions under which the inquiry has been made may be thus briefly stated:—

The cases were exclusively such as presented well marked signs of consumption. The diagnosis was, in every instance, made either by myself or by my colleagues at the Hospital for Consumption; and, as the cases had been under observation for a period, I venture to hope that no reasonable exception can be taken to it.

The inquiry was most carefully restricted to the period of health, except in reference to spitting of blood, the first occurrence of which, at whatever period, was recorded.

The patients were questioned alone, or in small parties of two or more at a time. The nature of the inquiry, and the limitation to the period of health, were carefully explained, and no person was included who was deficient in ready intelligence. As some of the questions required thought before the answer could be given, there was an advantage in asking them in parties, since time was given for consideration, and the example of each quickened the apprehension of the others. When any carelessness was evinced, the patient was removed; and when a similarity in the answers of two or more occurred, care was taken to repeat the inquiry and prevent routine answers. In every case each patient gave a separate answer.

The question of ages often presented a difficulty; many persons, and particularly the Irish patients, did not know their own age, and, it might be inferred, did not know the ages of their parents—and such were excluded. The periods of the occurrence of the diseases of infancy were determined approximately, or actually, as the case might be, so that they could be arranged under short periods—as under age 2, 2 to 5, 5 to 7 years, &c. The only difficulty arose in the cases in which the disease had occurred before their present recollection; but careful attention sufficed to arrange the ages under the above headings. When the patient was not sure that the disease had occurred, the answer was separately recorded, so that in the general returns the headings were—"Had the disease," "Had not the disease," "Does not know." The ages of the parents at the time of their death, and at the birth of the patient, were commonly known. The latter was ascertained by deducting the patients' from the parents' age at the time of this inquiry; and if the parent were dead, and the time of death known, the age at death and the subsequent period were added, and the same process was adopted. These calculations were always made by myself. In a few instances it was impossible to learn the age of the parent at either of the two periods, and such were classed as "Not known."

Questions which were of a private nature, as immorality in reference to intemperance, sexual abuse, masturbation, syphilis, and gonorrhea, were asked privately and carefully of each person alone, and they were, of course, restricted to male patients. Each case was identified by the initials of the name, the name of the physician, and the number in the hospital books.

In reference to the trustworthiness and value of the answers, I remark:—

1. That I believe them to have been given in good faith, and with care. In a very few instances, in females, there appeared to be a little confusion in the patient's mind upon some minute questions—as, for example, the age when the menses first appeared—but that was soon discovered in the course of so minute an inquiry.

2. The part of the inquiry the most liable to error is that of the diseases to which the parents, and brothers, and sisters, had been subjected; but it must be borne in mind that the questions asked were of diseases which would fix themselves upon the mind—as insanity, cancer, diseased bones and joints, scrofulous sores and scars, &c. The occurrence of consumption was limited to the relatives who had died from it; and by inquiry as to the age at death, and many other circumstances, erroneous opinions were corrected.

The state of the general health of the parents, both before and after the birth of the patient, was sought in only a general manner, such that any person would be able to supply. That of the health before the patient's birth must, of course, have been derived from hearsay; but in the degree in which it was recorded there can be no doubt but the patient must have been sufficiently informed.

3. There must be liability to error in all inquiries in which we depend for our information upon others, but the errors are, for the most part, those of omission. In such inquiries as the present the information, whether obtained for the case-book or the assurance office, must be derived from the patients alone; and hence I venture to claim for this inquiry as near an approach to truth as the case admits of. None of the patients included in this list belonged to the lowest class of the community, but nearly all were of the respectable working middle classes. No pains were spared in arranging the scheme of inquiry, the selection of the cases, the mode in which the questions were asked, and the sifting of the answers; and it may be fairly hoped that so long an inquiry, conducted upon a uniform plan throughout, would supply that tact and experience which would tend to accuracy. The whole record and the whole analysis have been made by myself.

4. Such questions as those of age, sex, residence, and occupation, can have value only as indicating the character of the persons who were submitted to inquiry. They are not adapted to show the prevalence of phthisis in the community under these various conditions. It is also a circumstance to be desired that similar details should exist in reference to the whole community including this

large class, or (if it were possible) of the community excluding those who have fallen, or will fall into phthisis, so that the true relation to a presumed higher degree of health, of the results now obtained, might be ascertained. At present the information is wanting, except in a very few of the subjects of inquiry, and hence the values obtained in this inquiry must chiefly stand as absolute quantities in a special class of cases.

5. In preparing the following analysis I have omitted any reference to such of the subjects of inquiry as had not elicited distinct and important facts, and have, therefore, much reduced the extent of the communication. The following is a list of the questions which were proposed to each patient. The returns embrace 600 males and 400 females;—

I.

Table of Questions.

Sex	No.	1
Whose patient		2
No. (Hospital)		3
Initials—Age—Married		4
Residence	Birth	5
	Most of life	6
	Last three years	7
	Unhealthy—why	8
Occupation	Before æt. 12	9
	„ æt. 21	10
	Since „	11
	Chiefly mental	12
	Unhealthy	13
	„ Dust—Fumes	14
	„ Long hours (name)	15
	„ Position of body	16
	„ Close, hot room	17
	„ Exposure	18
	„ Other causes	19
	„ Inten. applica.—æt.	20
Social Cond.	Aff. Mod. Bad	21
	Greatly changed, æt.	22
Nourished.	Dry nursed	23
	Under æt. 2, B. M. W.	24
	„ 10 „ „	25
	„ 16 „ „	26
	„ 21 „ „	27
	Since „ „	28
	Bad water	29
	Clothing (flan.)	30
Education.—B. M. G.		31
Anxiety Great	Nature of	32
	Before æt. 16	33
	„ 21	34
	Since „	35
Children	Æt. when 1st child born	36
	How many— L. & D.	37
	General health	38
	Abortions—No.	39
Habits previous	Moderate	40
	„ Regular or irregular	41
	Immoderate, æt.	42
	„ Drink—what	43
	„ Sex. Masturb. Emiss.	44
	„ Late hours, rising, bed	45
	Reformed, æt.	46
	Sudn. abst. from drink, æt.	47
	Teetotaller—Smoke	48

Temperament and Timbre in Health

Colour	49
„ Iris	50
„ Complexion	51
Frame	52
„ rounded	53
„ spare	54
„ height	55
„ weight (in health)	56
Susceptible and variable	57
Firm. Languid	58
Cold hands or feet	59
Temperam. S. N. Ly. Bil.	60

Parents, F. M. also B. S.

Nourished	61
Unsteady habits—F. M.	62
General health—F. M.	63
Æt. at death—F. M.	64
Æt. at patient's birth-F.M.	65
Health before do.—F. M.	66
No. of child. now liv. & dd.	67
Scrof. Sores. Scars. Glands	68
„ Eyes, joints, &c.	69
Consumption, Asthma	70
Insanity	71
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The returns are divisible into two large classes, viz., those which refer to the parents, and those which belong to the patients; and I propose to consider them in their order, after stating the indicating questions of the age, place of birth, and place of residence of the patients.

GENERAL QUESTIONS.

II.

1. *The Age of the Patient at the period of the Inquiry.*

This was recorded to the precise year; but for this analysis I have collated them into periods of under 20 years, and of each subsequent five years.

Age, . .	to 20	20 to	25	30	35	40	45	50	55	60
Females, per cent.,	15·8	20·8	25·5	15·8	10·	8·	2·	1·	·26	·52
Males, ,,	11·5	24·8	19·1	18·5	10·	6·6	5·1	2·5	1·1	·16
Average, ,,	13·	22·9	21·3	17·3	10·	7·1	3·9	1·9	·8	·3

The greater proportion were from 20 to 25, and from 25 to 30 years of age, in nearly equal proportions, and they comprehended 44 per cent. of the whole. About $\frac{1}{8}$ were under 20 years, and 3 per cent. were more than 50 years of age. 57 per cent. were under 30 years of age. The number of females under 20 years, and from 25 to 30 years, was greater than that of males. The average age of the whole was 29·66 years males, and 27·5 years females.

III.

2. *Place of Birth.*

Per cent.	London and denser Suburbs	Country	Ireland	Scotland	Channel Islands and Foreign
Females, per cent.,	26·5	66·	4·	·75	2·5
Males, ,, .	25·5	64·5	6·8	1·66	1·5
Both, ,, .	25·9	65·1	5·7	1·3	1·9

30 per cent. were born in London and its denser suburbs, and 65 per cent. in other parts of England, so that but few were born in the other parts of the kingdom and in foreign countries. There was great equality in the sexes.

IV.

3. Place of Residence.

—	Greater part of Life						The last Three Years				
	London	Country	Ireland	Scotland	Foreign	Various	London	Country	Scotland	Foreign	Various
Females, ...	36·2	52·5	2·75	·25	1·	7·	55·7	39·5	·25	·25	4.2
Males, ...	36·6	50·	3·83	·66	2·	6·8	52·1	40·6	–	1·8	5.3
Both, ...	36·6	50·9	3·4	·5	1·6	6·9	53·6	40·2	–	1·2	4·9

36 per cent. had lived in London during the greater part of their lives, 51 per cent. in the country, and 7 per cent. in various places; but, during the last three years, 53 per cent. had lived in London, and 40 per cent. in the country.

Hence it appears that the conditions under which the patients had lived were very diverse, so that no class peculiarities could attach to them, except that of their condition in life. The majority had been born and brought up in the country, but one-half had recently lived in London.

In reference to the condition in life it may be added that, with the exception of 8·8 per cent., all could both read and write, and in only 14·3 per cent. had they been badly nourished at any period of their lives. In both of these respects the females were a little less fortunate than the males.

V.

Education.	*Nourishment.*
Could not both Read and Write	Not well Nourished at same period
Females, per cent., . 9·7	13·5
Males, „ . 8·1	14·8

Hence it is probable that no number of persons could have been taken who would more fairly represent the active population of this island in reference to the conditions upon which health depends.

We will now proceed to cite the results of the inquiry; and in doing so shall divide them into two classes, viz., those affecting the parents and those affecting the individual.

QUESTIONS RELATING TO THE PARENTS.

VI.

Mortality per cent.

	Dead			Living
	Fathers only	Mothers only	Both	Both
Females, . .	53·2	47·5	28·5	25·7
Males, . .	55·1	45·5	27·8	25·3
Average, . .	54·4	45·9	28·1	25·5

We have already stated that the average age of the patients was 28·8 years, and it was seen that 54 per cent. had lost the father, 46 per cent. the mother, and 28 per cent. had lost both parents. In 25 per cent. both parents were living. There was singular uniformity in the two sexes.

VII.

Age of the Parents at their Death.

In the original returns we have noted the precise age at death, but for the purpose of this analysis we have thought it would suffice to collate them into quinquennial periods. The per centage is obviously not calculated upon the whole number of patients, but upon only the number of patients who had died.

Age of Patient,	20 to		25		30		35		40		45		50		55		60		65		70		75		80		85	
Sex of Patient,	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
Mothers, .	2·3	·58	5·5	6·8	3·5	8·1	11·5	13·5	12·6	17·	15·1	9·8	16·6	13·3	5·5	7·5	12·	5·1	5·2	6·3	5·5	6·8	2·	1·1	2·	2·9	8·8	7·5
Fathers, .	–	1·	1·6	5·5	6·	4·5	7·	8·1	13·1	13·7	15·3	12·2	14·4	9·1	6·7	10·7	13·1	11·2	6·4	8·6	8·	2·5	3·2	1·	3·2	5·6	7·6	9·1
Average, .	1·1	·79	3·4	6·2	5·	6·2	9·1	10·8	13·1	15·3	15·4	11·	15·6	11·	6·3	11·8	12·7	8·3	5·9	7·8	6·7	4·5	2·6	1·	2·6	4·3	8·2	8·3

Average of the Two Sexes of the Patients Combined.

Mothers, .	1·44	6·1	5·8	12·5	14·8	12·4	1·5	6·5	8·5	5·7	6·1	1·5	2·4	8·1
Fathers, .	·5	3·5	5·2	7·5	13·4	13·7	11·7	8·7	12·1	7·5	5·2	2·1	3·8	8·3
Average, .	·9	4·8	5·5	10·	14·1	13·1	13·3	7·1	10·3	6·1	5·6	1·8	3·4	8·2

8 per cent. were unable to answer the questions as to the age at death; of the remainder the largest quinquennial proportion died in the period from 40 to 45 years, but the true prevalent period was from 35 to 55 years, which alone included half of the whole deaths. 11 per cent. died under 35 years, and 27·0 per cent. lived to upwards of 60 years of age. Of the sexes of the parents there was the greatest mortality amongst the mothers up to æt. 55, but particularly in the period under æt. 30; and, in reference to the sexes of the patients, the mortality amongst the parents of the females was greater under æt. 30, and less than that of the males under æt. 55.

The average duration of life of the parents who had died was—mothers 48·2 years, fathers 52·9 years, both 50·8 years.

VIII.

	Females		Males	
	No.	Average Years	No.	Average Years
Mothers, . .	167	48·5	255	49·9
Fathers, . .	193	51·6	305	53·7
Both, . .	360	50·1	560	51·9

Health of the Parents.

This inquiry embraced two periods—that which preceded the birth of the patient, and that embracing the whole life; and it comprehended the whole of the 1,000 cases.

IX.

Feeble Health before Patients' Birth.

	Fathers	Mothers	Both included in the foregoing
Females, per cent., .	9·1	14·8	2·
Males, „ .	5·9	9·	1·9
Average, „ .	7·2	11·4	1·9

3·2 per cent. of the cases could not answer the questions.

X.

Feeble General Health.

	Fathers	Mothers	Both included in the foregoing
Females, per cent., .	15·7	25·	5·2
Males, „ .	11·1	19·	3·6
Average, „ .	13·	21·4	4·3

In the period preceding the patients' birth, 18 per cent. of one of the parents had had feeble health; and of that number the greater proportion, viz., 11 per cent., were the mothers. Both parents were similarly affected in 2 per cent. When the whole course of life was included the proportion increased, so that it became 34 per cent., and still the mothers exhibited a preponderance of 8 per cent. over the fathers. Both parents were feeble in 4·3 per cent.

Unsteady Habits of the Parents.

The inquiry was directed to ascertain the habits of both father and mother, but no special kind of conduct was particularized. In 23·0 per cent. one or both of the parents had been generally unsteady, but the proportion of the mothers was quite insignificant. There was a large preponderance of this evil in the parents of the male patients; the latter exceeding that of the females by 9 per cent.

XI.

	Father	Mother
Females, per cent., .	17·2	·5
Males, „ .	26·3	1·
Both, „ .	22·7	·8

Diseases of the Parents of the Patients.

Consumption.

I shall first cite the combinations of relationships which were given by the patients, and then make a summary of the prevalence

of mortality from this disease, under the different headings. I do not quote in the summary the number of each class of relatives who had died, but it was nevertheless ascertained, and also the fact as to whether the aunts, uncles, and cousins were on the fathers or the mother's side. I may, however, state that one female patient could count 10 relatives whom she had lost.

XII.

Relatives who have died of Consumption.

G.F.	G.M	F.	M.	B.	S.	U.	A.	Nos. and Sex of Patient		Per Cent.	G.F.	G.M	F.	M.	B.	S.	U.	A.	Nos. and Sex of Patient		Per Cent.
								F.	M.	Aver. of both Sex.									F.	M.	Aver. of both Sex.
–								2	1	·66			–		–		–			1	·22
	–							4	1	1·1			–			–	–	–		1	·22
–	–							1	1	·44			–			–		–		1	·22
–	–						–	1		·22			–				–	–	4		·88
–	–					–	–		1	·22			–		–		–	–	1		·22
–			–					1		·22			–				–		1	1	·44
–			–			–	–	1	1	·44			–					–	3	1	·88
–		–				–		1		·22			–						1	2	·66
–		–		–		–		1		·22			–	–						1	·22
–					–			2		·44				–		–	–		1	1	·44
–							–		1	·22				–		–		–		1	·22
–						–		2	1	·66				–		–	–			1	·22
	–					–		1		·22				–				–	2		·44
	–			–		–			1	·22				–				–	1		·22
	–						–	1		·22				–		–	–	–	1		·22
	–								1	·22				–	–	–			4	5	1·9
	–	–							1	·22				–		–			1	9	2·2
		–						20	19	8·5				–					1		·22
			–					26	30	12·3				–			–	–	1		·22
		–	–					9	3	2·6					–	–	–			1	·22
				–				22	34	12·3					–	–			1		·22
					–			27	35	13·6					–		–		1		·22
				–	–			16	12	6·1					–			–	1		·22
		–	–	–				3	2	1·1					–				1		·22
		–	–		–				4	·88						–	–			2	·44
		–	–	–	–			1	1	·44						–		–	1		·22
		–	–		–		–	1		·22						–	–	–	1		·22
		–	–			–	–	3	1	·88							–		5	8	2·8
		–			–			2	2	·88								–	8	8	3·5
		–		–	–			4	1	1·1							–	–	9	2	2·4
		–	–				–	1		·22							–		1		·22
		–		–			–	1		·22									13	4	3·7

This table is to be read continuously down the left and then down the right side. The – to the left hand of the Nos. indicates the relative or relatives who had died of consumption, whilst the

numbers opposite to it show the number of such relatives of the female and male patients respectively, and the average of them in the two sexes of patients combined. Hence, when there are several – they indicate that several relatives, of different degrees of relationship, have died in consumption; and the number of the combinations of such relationships is indicated by the numbers opposite to them. Thus, in the first line, the – is placed under G. F., or grandfather, and the numbers to the right show that in two cases of female patients, and in one case of male patients, the grandfather was the only relative who was known by the patient to have died from consumption; while in fifth line the – show that the grandfather, grandmother, uncle, and aunt of a male patient were the relations who had died of that disease.

XIII.

Summary of the Relationships.

Sex of Patient	Grandparent Per cent.		Parents Per cent.		Br. or Sis. Per cent.		Parents, Brs. or Sis. Per cent.		Uncles or Aunts Per cent.	
	Alone	All	Alone	All	Alone	All	Alone	All	Alone	All
Females,	1·7	4·5	13·7	26·5	15·7	25·7	·25	47·	5·5	13·7
Males,	0·5	1·6	8·6	17·3	13·5	21·6	·16	28·5	3·	6·
Average,	1·	2·8	10·7	21·1	14·4	23·3	·2	35·9	4·	9·1

The proportion in which consumption appeared in one relative only was very small, except in reference to the principal relatives—the parents and the brothers and sisters. In the parents it was 11 per cent., and in the brothers and sisters 14·4 per cent., and in each of these instances the relatives of the females had the larger proportion. The total proportion in which any set of relatives had died from the disease was, of course, much greater, so that one or both parents had died in 21 per cent., brothers or sisters in 23·3 per cent.; parents, brothers, or sisters in 36 per cent.; grandparents in 2·8 per cent. In every instance the mortality amongst the relatives of females was greater than in those of males; and in reference to the immediate relatives—parents and brothers and sisters combined —the excess was 19 per cent.

XIV.

Other Diseases.

Various				Presumed Scrofulous			
	M.	F.	Both		M.	F.	Both
Rheumatism, per cent.,	25·2	20·	22·1	Eyes & Ears,	1·25	1·3	–
Gout and Rheumatic Gout, per cent.,	7·5	7·	7·2	Joints, .	·25	·3	–
				Bones, .	1·	1·1	–
Cancer, „ .	3·5	3·	3·2	Spine, .	·25	–	–
Diabetes, „ .	·25	·5	4·	Glands, .	4·	5·	–
Insanity, „ .	3·7	4·6	4·3	General, .	3·	–	–
Liver, „ .	10·7	7·8	9·				
Kidneys, „ .	·7	1·8	1·4				
Brain, „ .	1·	2·1	1·7				
Ague, „ .	4·7	4·5	4·6				
Other Fevers, chiefly Typhus, per cent.,	4·7	4·8	4·8				
Asthma, „ .	13·	13·6	13·4				

As it respects the prevalence of presumed scrofulous disease, we might almost pass it over without comment; for, with the exception of the continuance of enlarged glands in 5 per cent. of the cases, the proportion was utterly insignificant. In only 8 per cent. were there any evidences known of the existence of that class of diseases.

Rheumatism and asthma were the most prevalent diseases—the former having occurred in 22·6, and the latter in 13·3 per cent.; and of the latter 9·4 per cent. were found in one or both of the parents only. Liver diseases and gout follow next in the order of frequency, and ague and typhus fever had each occurred in nearly 5 per cent. of the cases. Insanity has been noticed in 4·3, and cancer in 3·2 per cent.; whilst brain and kidney affections and diabetes had been still less frequent. The variations in the returns of the two sexes of patients were unimportant.

XV.

Consanguinity of the Parents.

In only 6 instances, or ·6 per cent., had the parents been related before their marriage; and of these an equal number belonged to each sex of the patients.

XVI.

Age of Parents at the Patients' Birth.

Age, .	15 to		20		25		30		35		40		45		50		55		60		65	
Sex of Patient, .	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.	M.	F.
	3·9	4·4	23·6	21·2	21·8	24·8	22·8	24·6	16·5	13·2	9·1	9·1	1·2	1·3	·9	1·	·36	–	–	–	–	–
	1·4	1·6	17·	14·	22·	24·9	23·5	24·6	16·8	15·4	11·	9·8	3·9	4·4	2·5	3·6	·9	1·1	·36	–	–	·28
Average, .	2·7	3·5	20·4	17·3	22·1	25·	23·3	24·6	16·8	14·4	10·1	9·5	2·6	2·9	1·7	2·3	·6	·56	·18	–	–	·14

Average of Both Sexes of Patients.

Mothers, .	4·1	22·4	23·2	23·7	14·8	9·1	1·2	·9	·18	–	–
Fathers, .	1·5	15·5	23·4	24·	16·1	10·4	4·1	3·	1·	·18	·14
Average, .	2·8	19·	23·3	23·9	15·5	9·8	2·7	2·	·59	·09	·07

37 persons did not know the age of one parent, and 33 of both parents, and in 24 cases the record was omitted.

The most frequent age was from 25 to 35 years, and that period alone embraced 47 per cent. of the whole number. Only 2·8 per cent. were under 20 years, and in 19 instances, or about 2 per cent., the age was from 55 to 70 years. Æt. 20 to 40 comprehended 81 per cent. of the whole. There was singular uniformity in the returns of the two sexes of the patients; but in reference to the parents, there were nearly thrice as many mothers as fathers under æt. 20; and between æt. 20 and 25 there was an excess of one-half on the mothers' side. After æt. 35, the proportion of the fathers preponderated.

XVII.

Number of Children in each Family.

No., . .	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	21	23
Females, per cent.,	2·7	4·	5·7	7·	8·	11·2	9·2	10·7	7·7	8·	5·5	5·7	4·2	·2	·1	·5	·5	1·	·25	·25
Males, „	3·3	4·3	6·6	6·1	7·5	9·5	10·8	10·1	9·3	9·3	6·6	5·6	3·5	·8	1·1	1·8	·3	·5	·16	-
Average, „	3·	4·2	6·2	6·5	7·8	10·4	10·	10·4	8·5	8·7	6·1	5·7	3·8	1·4	·6	1·2	·4	·8	·2	·12

The returns are very decided and striking. Families with 6, 7, and 8 children were the most frequent, and were each about 10 per cent. of the whole, so that the three comprehended nearly one-third of all the cases. There was a progressive and steady increase from families with 1, to those with 6 children, and a similar decrease to those with 13 children. In only 3 per cent. was there but one child; and families with 3 children or less formed only 15·4 per cent. of the whole; whilst in 5·2 per cent. the number varied from 14 to 23 children per family. Families having from 5 to 10 children constituted 56 per cent. of the whole. There was not any material difference in the proportion of the two sexes of patients, but the smaller families were more numerous in the parents of the males.

The average number of children to a family was 7·5; and it was almost precisely the same in both sexes of patients.

Mortality of the Parents' Children.

40 per cent. of the parents' children had died at the time when the patients were submitted to this inquiry, and of these an equal number was found in both sexes of patients. The average age of the patients has already been stated to be 28·8 years.

XVIII.

Order of Birth of the Patients among the Parents' Children.

Order, .	1st	2nd	3rd	4th	5th	6th	7th	8th	9th	10th	11th	12th	13th	14th	15th
Females, p.c.,	18·6	17·6	14·	10·7	7·1	9·1	6·	3·2	3·	1·	2·	1·3	1·	·32	·32
Males, ,,	21·	15·4	14·2	12·5	11·1	8·	6·2	3·8	3·6	2·	2·	1·4	·24	–	·24
Average, ,,	20·	16·4	14·1	11·8	9·4	8·4	6·	3·6	3·3	1·5	2·	1·4	·5	·13	–

Nothing could be more striking than these returns: 20 per cent. of the whole were 1st children, and from that there was an unbroken line of diminution to those who were the 10th children. Those who were 1st, 2nd, and 3rd children constituted one-half of the whole number. The latest was the 15th child; but after the 12th or 13th the number was quite insignificant. There was no material variation in the sexes.

Such are the principal facts which have been elicited in reference to the parentage of the patients, and I now proceed to state those which belong to the patient only.

QUESTIONS AFFECTING THE PATIENTS ONLY.

Questions affecting the Temperament.

Colour of the Hair.

The following table represents the frequency with which the shades of colour appeared, arranged in the order of intensity.

XIX.

Colour, . .	Flaxen	Light	Sandy	Light Brown	Medium Brown	Dark Brown	Chocolate	Black
Females, per cent.,	·25	15·2	1·25	2·2	34·	35·2	5·2	·25
Males, ,,	·33	27·1	6·	3·8	30·8	22·5	6·	2·1
Average, ,,	·3	22·4	4·1	3·2	32·1	27·6	5·7	1·4

The proportion of flaxen and black hair was quite insignificant, but the medium and darker shades of brown contributed more than two-thirds of the whole. If we arrange the shades into three classes, with the medium brown as the central, and the others as the extreme shades, we shall find that the three classes are constituted into nearly equal proportions, as in the next table.

XX.

Three Classes of Colour of Hair.

Colours, . . .	Medium	Dark	Light
	Medium Brown	Black Chocolate Dark Brown	Flaxen Sandy Light Light Brown
Females, per cent., .	34·	40·7	19·
Males, „ .	30·8	30·6	37·3
Average, „ .	32·1	34·7	30·

The females presented an excess in the medium and dark shades, whilst the light colours were twice as frequent in males as females. In reference to the separate colours the greatest disparity in the sexes occurred in the sandy hair, which was five times more frequent in males than females; but as the whole number was only 41, perhaps much importance should not be attached to it.

Colour of the Eyes.

For simplicity we arranged all colours under three heads, viz.:—grey, including blue; brown, including hazel; and black.

XXI.

Colour, . . .	Grey	Brown	Black
Females, per cent., .	69·2	26·2	·5
Males, „ .	77·1	20·8	·16
Average, „ .	74·	23·	·3

The proportion of black eyes was quite insignificant. The grey shades embraced 74 per cent., or three-fourths of the whole number. The brown colour was more frequent, and the grey colour less frequent in females than in males.

Colour of Complexion.

The inquiries had reference to the usual condition of the complexion, and, as already stated, in health only; and only two classes were recognized, viz., florid and pale.

XXII.

	Florid	Pale
Females, per cent., .	65·	34·2
Males, „ .	53·8	44·7
Average, „ .	58·8	40·7

The florid complexion was met with in 60 per cent., and the pale in 40 per cent. of the cases, and consequently the former much predominated. The florid was most frequent in the females.

Habits of Body.

The inquiry, as originally arranged, embraced four classes, viz., bony, muscular, rounded, and spare; but it was soon found that such a division was impracticable where we had to depend upon statements made in reference to an antecedent condition, and therefore the number was reduced to two—fleshy and spare.

XXIII.

	Fleshy	Spare
Females, per cent., .	61·9	38·1
Males, „ .	37·	63·
Both, „ .	46·7	52·9

The two classes were nearly equally divided, but the preponderance was in favour of the spare. There was, however, a very large excess of the fleshy habit in the females, and an equally large excess of the spare habit in the males, and hence the general average is unsatisfactory.

Coldness of the Extremities.

The inquiry had reference to general conditions only, and embraced both the hands and the feet.

XXIV.

	Hands alone	Feet alone	Both	Total
Females, per cent.,	1·5	23·7	37·7	63·
Males, ,,	7·	16·	25·1	48·1
Both, ,,	4·8	19·1	30·2	54·1

In 55 per cent. of the cases, coldness of the extremities had been habitual, 30 per cent. of which embraced both extremities, and 20 per cent. the feet only. It was much more common in females, except in reference to the hands only. The proportion in the two sexes was 63 per cent. of females, and 48 per cent. of males.

Temperament.

The information sought under this general heading had reference to the degrees of susceptibility or excitability, and the answer was derived partly from the answers of the patients, and partly from my own observation of the foregoing facts, and the aspect of the patients. The susceptible temperament largely prevailed, since it was found in 72·5 per cent. of the cases. It was well marked in both sexes, but somewhat more so in the females, since it was found in 74·5 per cent. of the females, and 68·5 per cent. of the males.

XXV.

Age of the Menstrual Epoch.

In 4 per cent. of the cases the menses had never appeared.

Aged years,	7,	10,	11,	12,	13,	14,	15,	16,	17,	18,	19,	20,	21,	24.
Per cent.,	·26	·53	4·5	6·2	11·4	18·2	18·2	15·1	8·	6·	3·9	1·3	·26	·26.

The information obtained under this head is very decided. The predominating periods were aged 14 and 15, at both of which the per centage was 36·4 of the whole. At 7, 10, 21, and 24 years the numbers were insignificant, but from 11 to 20 there is an unbroken line of increase and decrease, with the central periods just pointed out. 63 per cent. of the whole had the menstrual epoch at 13, 14, 15, or 16 years of age.

Irregularity of the Menses.

This was recorded under four heads, viz., much, frequent, little, seldom, and in their four combinations.

XXVI.

	Much	Frequent	Little	Seldom	Much & Frequent	Little & Seldom	Much & Seldom	Little & Frequent
Per cent.,	6·	2·5	29·	16·7	1·2	15·2	·5	·25

40 per cent. of those who had menstruated complained of some irregularity having occurred. The most frequent condition was a defect in the function, so that 29 per cent. found it deficient in quantity, and 16·7 per cent. too infrequent in time. In only 6 per cent. was the quantity excessive.

Leucorrhea.

This condition had been persistent in 42·2 per cent. of the whole cases.

Discharges of Blood.

The two conditions sought for were hemorrhoids and epistaxis.

XXVII.

Source, . .	Nose	Piles	Both
Females, per cent., .	9·7	2·2	·5
Males, ,, .	12·	2·	·5

Epistaxis had been somewhat frequent, viz., in 11 per cent. of the sexes combined, and with an excess in males over females. The other source was found in only 2 per cent. of the cases.

XXVIII.

Married and Childless.

	Married	Childless
Females, per cent., .	41·5	12·8
Males, ,, .	45·6	13·1
Average, ,, .	43·5	13·0

43·5 per cent. were married, with a difference of 4 per cent. in favour of the male patients. Of these 13 per cent. were childless, and there was no appreciable difference in the sexes.

XXIX.

Age when the First Child was Born.

Aged, .	16	17	18	19	20	21	22	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	42	43	45
Per cent. Females, .	–	4·5	6·7	6·7	6·7	10·	10·6	14·4	7·5	3·7	3·7	6·7	4·5	·7	3·	3·7	1·5	1·5	–	·7	·7	–	–	·7	–	–	–
Males, .	·4	·4	·4	2·2	4·4	5·8	11·6	8·1	7·6	8·1	12·6	8·6	6·7	4·4	4·	2·2	·9	·9	·45	1·3	–	·9	·45	–	·45	·45	–
Average, .	·28	2·	2·8	3·9	5·3	7·3	11·2	13·3	7·5	6·4	9·3	7·9	8·7	3·	3·6	2·8	1·1	1·1	·28	1·1	·28	·56	·28	·28	·28	·28	–

The most frequent age was 23, at which period 13·3 per cent. occurred. In the period including 20 and 25 years of age, 51 per cent. of the whole cases were recorded. The extreme ages were 16 and 45 years; but under age 20 only 9, and over 30 only 11·8 per cent. occurred, so that four-fifths of the whole had their first child when between 20 and 30 years of age.

There was a marked difference in reference to the sexes, so that it occurred in the females at an earlier age than in males. 18 per cent. of females had their first child under age 20, and 67 per cent. under age 25, but only 3·4 and 48·7 per cent. of males were recorded at the same periods. The excess in favour of females was thus 14·6 per cent. under age 20, and 18·3 per cent. under age 25.

XXX.

Number of Children in each Family.

Number, . .	1	2	3	4	5	6	7	8	9	10	11	12	13	14	19
Females, per ct.,	19·3	18·	9·6	9·6	7·5	6·2	7·5	3·4	3·4	2·	1·3	1·3	–	1·3	1·3
Males, ,,	16·3	23·1	10·9	10·9	7·5	8·8	5·4	4·6	4·6	2·1	·4	·8	1·2	–	–
Average, ,,	23·4	21·	10·4	10·4	7·8	7·8	6·2	4·7	4·7	2·	·78	1·	·78	·5	·5

The per centage is reckoned only upon those patients who were married and had had children. The number of one and two children in a family so greatly preponderated as to constitute 44 per cent. of the whole, and to cause a marked line of division between the numbers of two and three. The number of families with three children was not the half of those with two children; but from that number the frequency gradually declined, until ten children were found in a family. There were two families with nineteen children in each; but families with more than ten children were found in only $3\frac{1}{2}$ per cent. of the cases. Families with three or fewer children were met with in 55 per cent. of the whole. The average age of the persons included in this list was 28·8 years. The greatest number of families with one child was found in the females.

XXXI.

Number of Children now Dead.

	Now Dead
Females, per cent., .	43·7
Males, ,, .	31·7
Both, ,, .	38·

The number of children born in each family gave an average of 4·0, and it was precisely the same in each of the sexes. The division of the children into those then living and dead was not made in the early part of the inquiry, so that we have been compelled to exclude 7 per cent. from this analysis. Of the remaining number, 38 per cent. had died at the period of inquiry, and the excess on the part of the children of females was no less than 12 per cent.—43·7 per cent. of the children of females, and 31·7 per cent. of those of males having died.

General Health of the Children.

This inquiry was one of a general nature only, and such as any patient would be prepared at once to answer. In 43 per cent. of the cases the general health was feeble, and chiefly on the female side, for the precise per centage in the two sexes was 48.7 in females, and 37 in males.

XXXII.

Frequency and Number of Abortions.

Number of Abortions, . .	1	2	3	4	6	8
Per cent. of those who had miscarried, . . .	44·7	37·3	3·4	2·	1·	·5

Abortion had occurred in 46·2 per cent. of the child-bearing married women; the number of abortions which each of such patients had suffered varied from 1 to 8. Of these, one abortion was found in 44·7 per cent., and one and two abortions comprehended 84 per cent. of the cases included in this list.

XXXIII.

Smoking and Immoral Conduct in Male Patients.

	Smoke	Drank	Bad life for a period	Syphilis		Gonorrhea			Both Syphilis and Gonorrhea included in the foregoing
				Once	More	Once	Twice	More	
Per cent.,	48·	24·5	29·6	14·	2·	29·5	5·	4·	11·5

Nearly one half of the cases had smoked tobacco for various periods; one fourth of the cases had drank to excess; and 29·6 per cent. had led a bad life, in various ways, at some period; but usually for two or three years before twenty years of age.

Syphilis and *Gonorrhea* had occurred in 27·5 and 50 per cent. of the cases, of which 11·5 per cent. had both been found in the same person. The diseases had been contracted more than once in 11 per cent. of the cases; and, in a few, they had occurred five times and upwards.

Careful and private inquiries were instituted to determine the frequency of sexual abuse in early life, masturbation, and seminal emissions; and it was found that *sexual abuse* had occurred in 11·6

per cent., *masturbation* in 18·2 per cent., and presumed *seminal emissions* in 22 per cent. The two former were, for the most part, found before age 20, but the latter prevailed chiefly at a later period. The age at which these occurred was recorded in the inquiries.

Late Hours and Anxiety.

The occurrence of late hours was partly due to necessity, and partly to vicious habits. It occurred in 19·3 per cent. of the whole cases, but it was four times more frequent in men than women. The per centage was 7, and 27·5 in the two sexes. Anxiety was experienced in a degree fitted to injure the system by 22·2 per cent.; and of these, 32·5 were women, and 15·3 per cent. men; showing a very large preponderance in females.

Evils of Occupations.

70 per cent. complained of some injury inflicted by their occupation. This was made chiefly by the males, of whom 85 per cent. had suffered; but 49 per cent. of the females also attributed much importance to it. The causes assigned were very various; but certain were specifically inquired into, and others were included under one general heading.

XXXIV.

Cause, . .	Dust and Fumes	Long Hours	Bending Posture	Close and hot room	Exposure	Great application	Other Causes
Females, per cent.	2·2	20·5	13·5	19·	16·7	6·5	3·5
Males, ,,	24·8	34·	24·3	28·	42·3	6·1	8·3
Average, ,,	15·8	28·6	20·	24·4	32·1	6·3	6·4

The most frequent source of injury was exposure, which occurred in 32 per cent.; after which came closely long hours and close and hot rooms. Bending and dust were injurious in 20 and 15·8 per cent. respectively. In only 6·3 per cent. had there been unusually close attention to business; and an equal proportion complained of a miscellaneous list of evils, as standing, carrying weights, ascending steps, talking, bad odours, damp climate, steam, hot lime, pressure on the chest, and rowing. Several of this list of causes occurred to the same individual in a large number of the cases. These ill effects fell with very different degrees of frequency upon the two sexes. It has been already stated, that as a whole, they were much more frequent in men; but the exceptions in favour

of women were the most striking in reference to dust and exposure, in which the per centage in women was 2·2 and 16·7, whilst in men it was 24·8 and 43·2. The frequency of intense application was the same in the two sexes.

XXXV.

Inhaled or Imbibed much Mercury; Bled at the Arm.

	Mercury	Bleeding									
		How often, .	1	2	3	4	6	7	8	9	10	12
Females, per cent.,	5·5	Per cent. of	57·	21	10	5·	5	2·	–	–	–	–
Males, ,,	11·3	those who had	53·	20	15	4·7	1·9	·9	·9	·9	·9	1·9
Average, ,,	9·	been bled, .	54·4	20	13	4·7	2·7	1·3	·6	·6	·6	1·3

In nearly all the cases in which mercury had been largely taken there had been salivation; but in some instances the mercury was known to have exerted an injurious influence, although short of this by its employment in their occupation. In 9 per cent. this evil had occurred, but the per centage was twice as great in males as in females. General bleeding had been practised in 15 per cent. of the cases; and this also was much more frequent in males. The number of occasions on which the same patient had submitted to the operation varied from one to twelve, but in both sexes more than 50 per cent. of those who had been bled had only had one bleeding.

Perspired Very Readily. Not Worn Flannel Shirts.

In 25·4 per cent. the patients had evinced an unusual readiness to perspire throughout life, and the proportion was only slightly greater in males than females.

The inquiries in reference to the use of flannel were made under two heads, and included a period of six months; for, as the inquiries were not necessarily made on the day on which the patient first presented himself at the hospital, it was probable that the disease might have existed when the use of flannel had been so recently adopted. In 14·4 per cent. flannel had never been worn next to the skin, and the omission of it was more than four times more frequent in women than in men. 10·5 per cent. had only worn it six months or less: so that if we add the numbers together it may be stated that 25 per cent. had not usually worn flannel on the skin.

Feeble at Birth. Drynursed.

A few persons were unable to answer the first question, but in

nearly all the information was sufficiently exact. In 24 per cent. the patients had been born of feeble habit, and females had experienced it in the proportion of 5 per cent. higher than males. The proportion of cases which had been drynursed was insignificant, viz., 2·5 per cent.: and it was the same in both sexes.

General State of the Health.

This inquiry embraced the periods of childhood, puberty, and adolescence.

XXXVI.

	Good always	Not Good			
		To age 3	To age 14	To age 21	All the Life
Females, per cent.,	66·5	14·7	17·5	21·5	11·5
Males, „ .	86·5	7·8	7·6	8·8	4·8
Both, „ .	78·5	10·6	11·6	13·8	7·5

78·5 per cent. had always experienced ordinarily good general health, whilst 7·5 had never enjoyed it. Good health was 20 per cent. more common in males, and unbroken feeble health 7 per cent. more frequent in females. In reference to the periods of life when feeble general health prevailed, it may be seen from the table that it was nearly equally distributed over the three periods, but exhibited a tendency to increase in frequency as life advanced. At the period above, aged 21, it was recorded in 13·8 per cent. of the cases.

The General Appetite for Food.

This inquiry was made in the same manner as that just recorded.

XXXVII.

	Good always	Not Good			
		To age 3	To 14	To 21	All Life
Females, per cent.,	72·2	11·7	15·	19·7	10·7
Males, „	90·	5·6	6·	5·8	4·3
Average, „	82·9	8·1	9·6	11·4	6·9

The appetite had been generally good in 83 per cent. of the cases, with an excess in favour of the males of 18 per cent. It had never been good in 7 per cent., with a large excess on the part of the

females. The occurrence of defective appetite at the three ages of life varied from 8·1 to 11·4 per cent., and increased in frequency with advancing age. It was at all periods from two to four times more frequent in females than in males.

Understood Natural Delicacy of the Lungs.

This fact was recorded to have existed in 12·6 per cent. of the cases, with an increased per centage on the side of women, viz., 17 in women, and 9·6 in men.

Hemoptysis.

This question included the whole life, up to the period of inquiry, when the disease existed in the stage of consolidation or destruction, and the smallest streak was regarded as evidence of hemoptysis. In 32·7 per cent. this sign had never occurred, and it was so uniform in the two sexes as 32·2 per cent. in women, and 33 per cent. in men.

XXXVIII.

PREVIOUS DISEASES.

1.—*Sporadic.*

Age, . .	MEASLES										Not had
	To 2	2 to	5	7	10	15	20	25	30	35	
Females, per cent.	10·1	28·4	22·1	20·3	14·2	2·6	1·1	-	-	-	12·
Males, ,,	6·6	33·6	24·	22·6	10·1	1·7	-	·56	-	·29	18·5
Average, ,,	8·1	31·2	24·3	21·5	9·9	2·1	·48	·33	-	·16	15·9
	SCARLET FEVER										
Females, per cent.	3·5	15·3	14·4	22·1	29·7	6·8	4·2	·85	-	·7	57·5
Males, ,,	3·2	28·3	17·6	17·	17·6	3·2	6·4	·81	·16	-	70·6
Average, ,,	3·3	22·3	16·4	19·8	24·	5·	5·4	·8	·8	1·3	65·4
	SMALL-POX										
Females, per cent.	10·3	22·5	18·8	16·	14·1	9·4	1·8	·94	-	2·8	63·5
Males, ,,	9·7	26·	17·3	20·4	12·7	8·1	4·	·51	1·5	1·5	57·5
Average, ,,	9·9	24·7	17·8	18·8	13·2	8·3	3·3	·66	1·	1·	59·9
	HOOPING COUGH										
Females, per cent.	14·	34·	26·	16·	9·5	·5	1·	·5	·5	-	30·7
Males, ,,	10·	42·4	26·5	13·8	5·	1·2	·84	-	-	-	48·
Average, ,,	11·9	38·8	26·4	15·	7·1	·92	-	·23	·23	-	41·1

16 per cent. of the cases had not had measles; 65·4 per cent. had not had scarlet fever; 60 per cent. had not had small-pox; and 41 per cent. had not had hooping cough. Hence, of these diseases, measles was by far the most prevalent, and small-pox had been nearly as infrequent as scarlet fever. Measles, scarlet fever, and particularly hooping cough, were more frequent in males than females. The age at which these diseases occurred was not sought for in the early inquiries, but there are records of 1,587 instances applicable to our purpose.

In each of the four diseases the greatest number of cases occurred between two and five years of age, and the proportion was: hooping cough, 38·8; measles, 31·2; small-pox, 24·7; and scarlet fever, 22·3 per cent. The earlier period was more frequent in hooping cough, small-pox, measles, and scarlet fever, in their order; and the per centage varied from 12 to 3·3. Cases occurred of each of the diseases so late as from 25 to 30 years, and from 30 to 35 of measles; whilst cases of scarlet fever occurred up to five years later. 85 per cent. of the cases of measles, 61·8 per cent. of scarlet fever, 71·2 per cent. of small pox, and 92·1 per cent. of hooping cough occurred under age ten years. Four cases of measles, three of scarlet fever, and two of hooping cough were known to have occurred twice. Four cases had been inoculated for small-pox. In each of the diseases the number occurring under age two was greater in females than in males.

Especial attention was given to ascertain what ill effects had followed the occurrence of these diseases, and the returns are as follows:—

XXXIX.

	Measles	Scarlet Fever	Small-pox	Hooping Cough	Total
Number of Cases, . . .	741	404	354	531	2,030
Disease of the Eyes, per cent.,	·8	—	1·1	—	—
,, Ears, ,,	·26	—	—	—	—
Cough & Dis. of Lungs, ,,	·93	·71	—	1·1	—
General Debility, ,,	·4	·71	·84	·19	—
Had them severely, ,,	·4	·24	·28	·38	—
Disease of the Heart, ,,	—	·24	—	—	—
Dropsy, ,,	—	·49	—	—	—
Enlarged Glands, ,,	—	·24	—	—	—

In but one instance did any recognized ill effect follow in 1 per cent. of the cases, and, therefore, the returns are quite unimportant.

II.—PRESUMED SCROFULOUS AND ALLIED DISEASES.

XL.

	Females	Males	Total
Long continued diseases of the Eyes, per cent.,	4·2	4·6	4·5
„ Ears, „	·25	·16	·2
„ Bones, „	·5	·8	·7
„ Joints, „	1·	·5	·7
„ Glands, „	14·7	11·5	12·8
Marasmus, „	·25	1·	·7
Rickets, „	·5	—	·2
Peaked Chest, „	1·	1·1	1·1

In 21 per cent. there had been some member of this class of diseases; and of that 12·8 per cent. alone were enlargement of the glands, and 4·5 per cent. long-continued disease of the eyes; and hence the affections of the ears, bones, and joints, marasmus, rickets, and peaked chest were too infrequent to assume any importance. The affections of the glands were about 3 per cent. more frequent in females than in males, whilst those of the eyes were equally prevalent in the two sexes. No case of rickets was found in the 600 male patients.

XLI.

III.—OTHER DISEASES.

	Females, Per cent.	Males, Per cent.	Average
Inflammation of Lungs,	20·2	14·3	16·7
Rheumatism, or Rheumatic Fever,	14·	15·3	14·8
Fever, chiefly Typhus,	10·	6·6	8·0
Diarrhea,	8·5	8·	8·2
Liver Disease,	4·5	4·1	4·3
Ague,	3·7	6·8	5·6
Fits (nearly all in infancy),	3·5	2·3	2·8
Brain Disease,	1·0	·16	·5
Kidney „	·5	1·5	1·1
Dysentery,	·7	1·1	1·
Cancer,	·25	—	·1

No instance of insanity or diabetes was found in the 1,000 persons, but some one or more in the list of diseases now recorded

was found in 63·8 per cent. of the cases. The most prevalent diseases were inflammation of the lungs and rheumatism, or rheumatic fever; the former of which had occurred at some period of life in 16·7, and the latter in 14·8 per cent. The former was 6 per cent. more frequent in females, and the latter was a little more frequent in males. After these came fever—chiefly typhus—and frequent diarrhea, each of which occurred in about 8 per cent., with an excess of 3·4 per cent. of fever cases in the females. Ague had occurred in 5·6, and liver affections in 4·3 per cent., with an excess of ague cases in the males. Fits were recorded in 2·8 per cent. As dysentery, kidney affections, brain disease, gout, and cancer had occurred in 1, or less than 1 per cent., we need not enter into any detail respecting them.

Such is a summary of the facts which have been elicited in reference to the second part of the inquiry; and we will proceed to state, in a few words, the leading truths which this extended investigation suggests.

The first question which arises is that of hereditary transmission, either in the sense of absolute transfer of the elements of the particular disease from the parent to the child, or the communication of a state of the system in which disease in general, and this disease in particular, may probably originate. There is a wide difference in these two ideas, and yet it cannot be doubted that they both exist in the minds of various professional men at this day. The former is the older one, and that which the increasing knowledge of our day has rendered less tenable than was formerly believed, since the idea of the transmission of the germs of disease *in utero* is now more strictly limited to such specific diseases as syphilis. Yet it must be admitted, that whilst the growing feeling of the day is in favour of a theory which only implies a defective constitution, there is an under current of belief that this assumes a specific direction in the production of this particular class of disease. Hence, whilst there is a clear distinction in the two theories in statement, there is far more oneness in belief We will look at them in both aspects.

Feebleness of the general health of the parents existed before the birth of the patient in one-fifth, and throughout life in one-third of all the cases. It is quite certain that the former statement would be under the truth, since the child would only know of such marked deficiency of health as would, in after years, have been

matter of frequent conversation. It is also necessary to consider the two periods together; for although it may be objected that the condition of the health of the parent, after the birth of the child, in no way concerned the health of that child, there is strong presumptive evidence that general feeble health throughout life indicated a condition of the constitution below that of health, and hence would have existed, although it might not have demonstrated itself before the birth of the child. We shall, therefore, more nearly approach the truth if we take the larger per centage to represent the true state of the system, and affirm that one-third of the parents had feeble general health.

The mortality of the parents was such, that one-half of one, and more than one-fourth of both, were dead at the period of the inquiry. Hence, in three-fourths of the cases, one or both parents had died. In the same manner it is shown that in only one-fourth of the cases were both parents living. The value of these facts can only be estimated by considering the age of the child at the period of inquiry, and the age of the parent at the period of death; for it is evident that, as the child was younger or older, so would the parents, in the natural order of things, be living or dead. The average age of the patients, at the period of the inquiry, was 28·8 years. The age of the parents at their death, as ascertained by direct inquiry, was such that one-half of them died between 35 and 55 years of age—that is to say, in middle life; but still a larger proportion died after than before the period, so that some lived to upwards of 90 years, and more than one-third of the whole lived to above the period when the majority died. The proportion of earlier deaths was about one-third of the latter number, and was therefore inconsiderable.

The influence of the acquired causes of disease in the parents has not been inquired into beyond the comprehensive question of unsteadiness of life; and the frequent occurrence of this cause has considerable importance. Of the diseases which had occurred, other than phthisis, we may remark that only rheumatism and asthma were sufficiently frequent to attract attention; and it cannot be presumed that they had any very direct bearing upon the general health of the children. Such diseases as gout, cancer, and various kinds of fevers, were unfrequent; and, with the exception of liver disease, the others were not worthy of attention. Hence, we do not think that these diseases of the parents had either an indicative or causative value in reference to production of phthisis in the child.

It is of interest to remark how prolific were the parents of phthisical patients, for an average of $7\frac{1}{2}$ children to each family is much greater than that of the general community; and also that, in so large a proportion, the patient was the first or second child.

The importance of the first fact, therefore, extends chiefly to the early period of life, and does not show that it had produced a feeble state of the vital powers, such as might have been inferred if the patients were chiefly the last children born. Neither are we entitled to affirm that the powers of the parents were immature when the patients were born; for the age of the parents, at the birth of these children, shows that they were not largely the product of very early marriages, but they were born at a period of life when, in this country, the body is presumed to have approached maturity. It is true that we have shown that a large proportion of the parents had feeble general health, and therefore it might occur that their period of maturity had been deferred beyond the ordinary period; but if debility of system of the parent be presumed to be a predisposing cause of phthisis in the child, and that debility had existed throughout life, it would be more probable that its effect would increase as life advanced, and be more evident in the children of later years.

Hence, whilst these facts have great interest, I do not think that they help us to any affirmative views of the hereditary nature of phthisis.

The mortality of the children was considerable; since, when the average age of the parents was 28 years, 40 per cent. of the children had died, and that fact would imply the existence of a feeble state of the system.

We may now turn to the other aspect of the question, and show how far a direct transmission of disease might have occurred in the cases in question. It is evident that the solution of this question must rest alone upon the occurrence of phthisis in the parents, since from them alone could the disease have been transmitted. To introduce the occurrence of the disease in the next relatives, viz., the brothers and sisters, would be valueless and superfluous; for, if the proposition were thus—because the brothers and sisters of the patient, as well as the patient, had phthisis, there is a presumption that they had derived it in common from their parents, it would prove nothing beyond what could be derived from the parents alone, by ascertaining their mortality from phthisis, unless we are at liberty to infer that phthisis is a disease which may be

communicated through the parent to the child, without the parent having suffered from it—an inference which, although supported by a few facts, has hitherto found no place in the idea of the hereditary transmission of the disease. So, in like manner, we may discard inquiries into the occurrence of phthisis in the uncles, aunts, and cousins, since we have direct testimony as to the parents themselves.

In only one-fifth of the cases has either of the parents died of phthisis, although they had all lived until middle life; and hence we may support the statement of Professor Walshe, derived from fewer facts, that "phthisis, in the adult hospital population of this country, is, to a slight amount only, a disease demonstrably derived from the parents." The proportion of deaths from phthisis in these 1,000 cases was, however, somewhat higher than that which is found in the community as a whole.

As a general result of the inquiry under this head, we may affirm that phthisis is not necessarily nor usually a disease directly transmitted from the parent to the offspring, but that in a large proportion of phthisical patients the parents and brothers and sisters had experienced feeble health, and a somewhat lessened duration of life. There was not, however, a majority of the cases so connected.

The next question of interest is the liability of females over males to many of the conditions which have been embraced by this inquiry. There is a singular unanimity in this respect with regard to the most important subjects. Thus, in reference to the parents, more mothers than fathers had children early, had feeble general health both before and after the birth of the patient, and had died early. Of the patients, more females than males had mothers who died early; had most parents, brothers, sisters, and other relatives who had died of phthisis; had parents with one child only; had experienced feeble health and defective appetite throughout life; had been believed to have delicacy of the lungs; were young when their first child was born; had children of feeble health; and had lost most children. Of the less important questions it may be added, that more females than males had suffered from anxiety; had had measles, scarlatina, and hooping cough; had not worn flannel upon the skin; had a very defective education; were of a susceptible temperament; had brown eyes, florid complexion, and fleshy habit; and had experienced coldness of the extremities. Such a preponderance of evils in one sex is most striking, and is

not paralleled by any observation hitherto recorded. It also shows how great is the mother's influence upon the health of the children, and how much greater watchfulness should be exercised over the female part of the population.

3rd.—Of the group of questions which have a direct bearing upon the health of the patients, it may be remarked:—

1. That debility of the general system, both at birth and in later life, was not a marked feature, since two-thirds had enjoyed good health and appetite through life; but the remaining proportion of one-fourth had been feeble from birth. Of the periods during growth, that from æt. 14 to 21 had a preponderance of cases in feeble health, but only to a moderate degree. Known delicacy of the lungs was found in only one-twelfth of the cases; coldness of the extremities was experienced in one-half of the cases; and there was a well-marked tendency to free perspiration. Leucorrhea was prevalent.

2. The menses did not appear too early on the average, neither were they in excess either in time or quantity. Early marriages were not common, but the health of the children was bad and the mortality great in one-half the cases. Abortions were frequent, and the patients were prolific beyond that of the general community. Sterility was found in one-eighth of the married cases.

3. Immorality of life in the males, for a limited period, was frequent. Syphilis and gonorrhea had occurred and recurred frequently. Masturbation and seminal emissions had been common. The evils attending occupation were very considerable and important, since in two-thirds of the cases they were complained of. Of these, long and late hours, close and hot rooms, and exposure, were the most frequent. Anxiety was prevalent.

4. Of the sporadic diseases of infancy, by far the most frequent was measles; and neither scarlatina nor small-pox had occurred in one-half of the cases. The occurrence of each of the diseases in adult life was recorded, but it was very unfrequent. Evils resulting from these diseases were very insignificant.

5. The occurrence of the evidence of scrofulous disease was very rare, except in the instance of enlarged glands; but it is possible that a different result might be obtained from inquiries made at the children's hospital and in institutions where scrofulous cases are congregated. At the Hospital for Consumption there is shown to be no general or necessary connexion whatever between marked scrofulous diseases and phthisis.

6. Of general diseases, those only which were frequent were inflammation of the lungs and rheumatism.

7. The occurrence of consanguinity in the parents, and of dry-nursing was scarcely found. Asthma was found in one of the parents, and chiefly the mother, somewhat frequently.

I have not, in this summary, entered into a minute analysis of the results obtained in this inquiry, but have selected only those which occupy a prominent place, or may be grouped together. The results obtained will bear, it is hoped, an importance beyond that to which I have applied them, and will be of greater value when similar inquiries shall have been made upon other large sections of the community. Whilst it has been shown that many conditions have less importance in connexion with phthisis than has heretofore been believed, there is much evidence to show that the disease is frequently allied with a state of system defective in vital power and resistance, both of the patient, and his parents, and his children. The large proportion in which none of those states of health could be discovered, is, however, sound proof that phthisical patients are a mixed class, and that the disease arises under very diverse conditions. As to the bearing of this inquiry upon life assurance, I think it may be inferred that there is no such oneness of type of constitution that the most minute historical research could be a sufficient guide as to the future probability of the occurrence of consumption. There is clearly great diversity of causes leading to that issue, and hereditary influence can only be regarded as one of them. If the inquiry could have determined the proportion of persons derived from consumptive parents who would themselves become consumptive, it would have been more to this purpose; but such an inquiry is manifestly impossible. The only safeguard to life offices is, I believe, the estimation, by careful examination, of the degree of health of the proposed assurer; and, above all, the careful examination of the chest by those whose duties make them especially familiar with the subject, and particularly with the early conditions which precede the ordinary manifestations of lung disease.

Art. III—*Cases of Syphilitic Insanity and Epilepsy.* By James F. Duncan, M.D., M.R.I.A., Fellow of the King and Queen's College of Pysicians in Ireland, Physician to the Adelaide and Simpson's Hospitals, &c.

Among the remote constitutional effects of the venereal poison, diseased states of the nervous system occupy a prominent position; yet our knowledge of this most important pathological fact is of very recent date. The older writers on syphilis are entirely silent on the subject; and the treatises and text books in ordinary use, in the present day, either ignore it altogether, or dismiss it with a very cursory remark. Many circumstances have probably concurred to cause the connexion subsisting between the symptoms and their cause to remain so long unrecognised. The late period at which they usually set in, the obscurity in which their origin is involved, and the difficulty of ascertaining the earlier history of a case which may have lasted for years, all combine to account for an omission so little to be anticipated. It is to the late Dr. Todd, of London, and to Dr. Thomas Read, of Belfast, that we are principally indebted for bringing the subject, in its extended relations, prominently before the profession. The former, in a clinical lecture, published in 1851, noticed not only epilepsy and paralysis, as resulting from syphilis, but also a contracted and rigid condition of the flexor muscles of the forearm, accompanied with numbness, which yielded to treatment of a specific nature, after having resisted other remedies. And Dr. Read, whose interesting paper appeared in the February number of this Journal in 1852, further notices the occurrence of insanity, in addition to other forms of nervous disorder.

Since that period numerous essays on the subject, by different authors, have appeared in the journals. Professor M'Dowell published a case in the *Dublin Hospital Gazette*, in April, 1854; Dr. Chapin, an American writer, details a number of cases in the *American Journal of Insanity*, for January, 1859, in which he traces mental disease to a syphilitic origin; and Messrs. Hutchinson and Jackson, of London, have published a very instructive series of papers in the *Medical Times and Gazette*, for 1861, in which they give the particulars of several cases, illustrative of the protean forms of nervous disease originating in this source.

These references plainly prove how very short a time has elapsed

since the members of the profession became fully aware of the true nature of these affections, and to consider their occurrence as a part of the natural history of syphilis. Even yet there is reason to believe that the broad fact of the existence of such remote consequences, as an effect of the venereal poison, is not so well known to the generality of practitioners as it ought to be. Its importance is too obvious to require to be insisted on, affording the only hope of recovery or amelioration in a class of cases which usually resist ordinary remedies, but which yield, with more or less facility, to the treatment appropriate to the primary affection.

Nor is it certain that cases of this kind are by any means rare; the instances already published would rather lead us to the opposite conclusion. There is abundant reason to believe, that when the attention of practitioners has been more fully directed to this subject, they will be enabled to trace a latent syphilitic taint in many instances, where but for a knowledge of this fact, it would have been supposed that the venereal poison had either worn itself out or been eradicated.

It is not, I think, too much to say, that our knowledge of this important subject is as yet quite in its infancy. What may be the precise nature of the lesion, in these cases, is altogether unknown; whether it be the same in all cases, or whether it may vary; whether it be due to the mechanical pressure of an internal node, producing irritation and inflammation of the arachnoid and subjacent nervous matter, or to some peculiar deposit, corresponding to what takes place from the same cause in other organs, or to some alteration in the nutrition of the part depending upon some change in the arteries of the brain. These are questions which seem likely to occupy pathologists for a considerable time, and to furnish a wide and interesting field for future research. Nor are the mental phenomena observable in these cases a less interesting study to those practitioners who make the phases of disordered intellect their special care. Does the form of insanity which occurs under these circumstances exhibit a uniform character? If not, how does it vary, and is there anything, in the particular aspect of each case, to account for the diversity? Can we predicate with any certainty the nature of the predisposing cause from the phase which the insanity assumes; are melancholia and delirium equally the results of the syphilitic cachexia? Does it produce the tendency to suicide? Does it alter the moral character of the man, making him who was open, affectionate, and generous, reserved, suspicious,

and estranged? Does it lead to the idea of plots and secret conspiracy; or of notions of exalted rank, unbounded wealth, and superhuman power. These, and a multitude of other questions equally interesting to the medical psychologist, must be left unanswered until we have a greater collection of accurately observed facts to draw conclusions from, which shall be deserving of notice.

The following cases are given as a very small instalment to the elucidation of this important subject. They possess, I think, sufficient interest to be laid before the profession; and the success which has attended the treatment, though not all that might be desired, affords the hope that in other instances, when the affection may be earlier recognised and more promptly treated, we shall have abundant reason to be satisfied with the result.

A.B., the younger son of a gentleman of property, some twelve years ago, on arriving of age, contracted syphilis, and placed himself under the care of the late Sir P. Crampton, who gave him mercury, and kept him under it for a long time. His system was severely affected by the medicine; he had sores on the penis, and buboes; but, as far as he can recollect, the latter came on afterwards from exercise in the country. He had syphilis two or three times about the period referred to, but never subsequent to the treatment by Sir P. Crampton. Despising the too common opinion entertained by our countrymen of the unsuitableness of business occupation to gentlemen of his position, he embarked his capital in a trading concern, as emigration agent to America and other places. Meeting with losses in this, he determined to leave the country, and landed in Australia, where he married a healthy woman, beneath him in rank, by whom he had several children, all healthy. Believing that his marriage would be displeasing to his mother and other members of his family, from whom he hoped to receive pecuniary assistance, he determined to conceal it; this, together with the increasing difficulties of maintaining himself by the precarious resources of an adventurer's life, must have preyed heavily on his mind, and predisposed him to the attacks of nervous disease, from which he subsequently suffered. He accepted an appointment in the police, and was necessarily exposed to much hardship. In the year 1854 he had an epileptic fit, but of so slight a character that he paid no particular attention to it; he was unable to account for its occurrence by any special exciting cause. Two years after he had a second, much more severe; and in a year and

a-half later, somewhere about 1858, a third. This last was so severe that he remained insensible for three weeks, and his jaws were closely contracted, so that it was impossible for any food, liquid or solid, to be got down by the mouth, and nutrition had to be sustained wholly by enemata. It was altogether three months before he could leave the hospital. On his recovery he was discharged from his situation, and had to return home. On the voyage he was so ill, I believe from a return of the epileptic fits, that a bet was confidently made, that he would never reach England alive. His wife was left in the colony, to support herself by her own exertions, while he endeavoured to obtain from his relatives the means of maintaining himself again in some other industrious pursuits. Here, according to his own account, he met with innumerable disappointments and delays; at one time a provision would be promised him, but when the time came for its performance, something would occur to prevent it, and so on. Meanwhile, acting, I presume, upon some expectation of this kind being about to be realized, he wrote out for his wife and family to come over. When they arrived they found him still in an unsettled state, without a home to take them to, without means for their support, and without an honourable recognition on the part of his family; his marriage was, therefore, still carefully concealed from every one. During the interval that elapsed, between his return to England and the arrival of his wife, his own health was so impaired that he had to place himself under medical treatment; he was pale, depressed in spirits, suffering from necrosis of the olecranon, nodes upon the cranium, and occasional epileptic fits; for these symptoms he was under the judicious treatment of Mr. Smyly.

About the latter end of August, 1861, as he was crossing over from Liverpool to this city, he suffered from an attack of epilepsy, which was succeeded by a paroxysm of violent mania, in which he had to be restrained by force, to prevent injury either to himself or others. I believe this was the first occasion when any of his fits had been followed by mental derangement. On the 2nd September he was transferred to Farnham House Private Lunatic Asylum. I was absent from home at the time; but I understand that when carried in, such was his state of deep insensibility, that no person who saw him believed that he would leave the place alive. From this, consciousness slowly returned; but the next night he had another attack, of extreme severity, threatening immediate dissolution. For the next six weeks the attacks recurred at short intervals,

with varying intensity, each attack being succeeded, not only by lassitude and exhaustion, but by more or less marked delirium. I saw him about the commencement of October. In the intervals between the fits he was very rational, but desponding; his system, apparently worn out by fatigue and disease, and bearing the unmistakeable evidences of periosteal inflammation on both sides of the head. There had appeared, too, paralysis of the sphincters, after some of the later fits. Knowing that he had been, for a long time previous to admission, under treatment for tertiary syphilis, and seeing his exhausted condition, I at first thought that any further recourse to mercury would be out of the question, and was satisfied with continuing the treatment previously in use, namely—diffusible stimulants internally, and derivatives to remote organs, to relieve cerebral congestion. Finding, however, that these, though beneficial, failed to remove the liability to a recurrence of the fits, I thought it right to consult Mr. Smyly, his previous medical adviser, as to the propriety of resorting cautiously to the use of mercury. He said it might be tried, but that he thought it would not effect the purpose, as the attacks would be sure ultimately to return as bad as ever; he recommended, however, the use of the citrate of strychnine and iron, as a means of removing the incontinence of urine This was accordingly tried on the 11th of October, and with the happiest effect; the unpleasant symptom immediately ceased, but the medicine brought on an attack of diarrhea, which led to its discontinuance on the 21st; and on the 26th I resolved to put him on a combination of blue pill, sulphate of quinine and taraxacum—watching its effects. The mercury was given only in one-grain doses, once a day; the nodes on his temple were painted, as they had previously been, with a strong solution of iodine, and his strength supported by nutritious, but not stimulating, regimen. Under this treatment he steadily improved, no unpleasant symptom occurred, his mind cleared up, he had no return of the fits, and he was able to leave the institution early in December. His subsequent history is so far satisfactory. Notwithstanding the continuance of the many sources of mental disquietude already mentioned, he has remained free from any return of his malady, except on two occasions, each of them very trifling; the first, on the 1st of June, lasting only three minutes, and brought on by a dispute with his landlady; the second, the next day, which was equally short, and did not occasion him to fall. Since then he has succeeded in getting some sort of a settlement made with his

friends—he has confided the secret of his marriage to his elder brother, but not to his mother—and he has embarked in an anxious and labourious undertaking in the West of Ireland, connected with the curing of fish.

I am far from looking upon this case as a permanent recovery; but I think the result, when compared with the state in which he had been before the treatment was commenced, is extremely satisfactory. What part of the benefit he experienced is to be attributed to the mercury is a question upon which differences of opinion may be entertained. Undoubtedly the fits had ceased for some days previous to its use; and we all know that epilepsy often undergoes a spontaneous remission which is set down to the operation of some new plan of treatment instituted for the patient's benefit. In this instance I do not think there was anything acting on his mind or his peculiar circumstances to account for the amendment; and I cannot but consider the long interval of exemption, and the extremely mild character of the subsequent attacks, as due to the action of the mineral. His system never was fully brought under the influence of the remedy. Whether it would have been right to have pushed it further than it was used, or whether a more speedy method of administering it should have been resorted to, are points which I will not undertake to determine. I confess I was afraid, in a constitution such as I felt I had to deal with in this instance, either to give it very freely or to push it very far. But whatever I might have felt at liberty to do, had the patient remained in the asylum, I felt it would be highly improper either to retain him there when his reason was restored, or to attempt to carry out a plan of treatment, afterwards, which might have been rendered particularly injudicious by any irregularity on his part in his mode of living when he became his own master.[a]

Case II.—C. D., aged 42, admitted into Farnham House November, 1861. Contracted syphilis sixteen years ago, which was treated, as far as can be ascertained, by mercury, given unsteadily, and probably without proper precautions. He was supposed to be

[a] Since the foregoing was written, the gentleman called on me twice, November 15 and December 16, 1862, having returned from the West, disappointed in his enterprize, and heavily out of pocket; since his return to town he had one slight attack, on the 13th November, which lasted only a few minutes.

I ought to add, that I consider the syphilitic taint to be due, not to the original infection treated by Sir P. C., but to a fresh exposure, as his wife, *subsequent* to his leaving the asylum, exhibited a well marked venereal ulcer on the right cheek and within the right nostril. She had been previously quite healthy, and the children were perfectly healthy.

cured. Subsequently he married respectably in life, without any ill effect, but had no family. His wife, to whom he was much attached, died a twelve-month previous to admission. His father died in January, 1861, leaving him heir to his landed property and sole executor to his will. This arrangement was made under the impression that he was a first-rate man of business, and that from his living on the spot he would be better able to fulfil the duties of the will than either of his brothers. He very soon, however, began to exhibit unmistakable evidences of insanity, particularly in the random and extravagant way in which he acted—purchasing cargoes of goods totally unconnected with his business, which was that of a miller; buying worthless horses, at a large price, which he did not want, and selling other valuable animals at a dead loss. His conduct in Liverpool was so obviously that of a lunatic, that a friend, seeing the state he was in, had him placed in a private asylum near that town, about the month of June, 1861. He was transferred to Ireland under the idea that he was labouring under softening of the brain, and not likely to recover. At the time of his admission his state was as follows:—He was a large, athletic-looking man, of a slightly florid aspect, with a pinkish-yellow tinge in his complexion, unpolished in his manners, talking a great deal very freely, and in rather a loud tone of voice, but exhibiting no disposition to violence; amiable in his temper, with a large appetite, and disposed to eat rather fast. He had an ugly-looking boil on the right cheek, and an eruption of herpes both on the wrists and chin; on the right temporal bone a faint appearance of a node was to be detected, which was tender on pressure. His conversation partook largely of the character attributed by phrenologists to the organ of wonder. He was constantly boasting of his property and prospects. Of a horse, that he bought for £50, that was fired for spavins at two years old, but which otherwise would have been worth £100—that he has trotted him five miles in 15 minutes—that on one occasion he beat him as hard as he could to make him break his trot, but never succeeded; and that he would bet he would trot 20 miles in one hour, and never break out of his pace. That he intended to study for the bar, and would certainly become Lord Chancellor. That he fully expected to be married to Sir James Graham's daughter, but out of respect to his memory, then only recently dead, he would put it off for three months, &c. Taking into account the fact that a node could be detected on the side of the head, and the possibility that either he had contracted a fresh infection, or that the old

poison, not properly eradicated, had been lighted into fresh activity by dissipation and excitement, I determined to try the effect of a mild mercurial course. He was accordingly put upon a combination of Plummer's pill, quinine, and opium, at night—with sarsaparilla and aqua calcis, in the morning. This was continued for six weeks without any action on the system, but with a very decided improvement in his appearance and mental condition. He no longer talked in the foolish extravagant manner he had done; all his delusions disappeared; his general demeanour became quieter, and his tone more natural. The herpetic eruption, though not entirely eradicated, faded away; and the node lost its prominence, and no longer gave him pain when touched. Satisfied with this improvement, I thought it better to lay aside the treatment, and resort to other remedies which would be free from the objections to which a protracted use of mercury was liable; I am not certain that I acted wisely in this course, for he remains to some extent in an unsatisfactory condition still. He is indolent in his habits—fond of reading, particularly novels; but unwilling to go out or take exercise; he is somewhat bloated in look, with a hepatic tinge of complexion; and is not as attentive to his dress and personal appearance as I am sure he would be, were he perfectly recovered. His delusions are all gone, and his mental powers are fair enough, but the cutaneous eruptions, which were never entirely removed, have again become troublesome. I have, therefore, thought it advisable to resume the former treatment, and push it to a greater extent.

Both the cases just detailed appear, undoubtedly, to be due to the tardy action of an obscure venereal taint. Whether the case now about to be mentioned belongs to the same category is not so certain. In him, as I have been informed, there existed an hereditary predisposition to insanity, on both sides of his family; and there certainly was no trace of any actual symptom to be discovered in his person to prove the fact of his having suffered from it; yet there are grounds of suspicion sufficient to warrant the inference that this is really the essence, or at least a principal part of the essence, of his malady :—

E. T., aged 37, a gentleman in affluent circumstances, about eight years ago proposed marriage to a young lady, whose mother refused to entertain the idea on the ground of her then extreme youth. The gentleman went abroad, and four years afterwards renewed his suit, and was accepted. Two miscarriages, at an early

period of pregnancy, followed in the first year and a-half after marriage. What may have been the precise condition of the fetus I have been unable to ascertain. Neither the lady nor her husband appears to have suffered any symptoms of a syphilitic character. After the second abortion the gentleman began to exhibit such unmistakeable evidence of mental disease, that, knowing the hereditary taint in the family, the lady was advised to live separately, in order to avoid having a family. From eccentricity he passed through the several stages of reckless extravagance, then of wild phrensy—breaking furniture and everything within his reach ; then becoming dangerous to himself and others; and, finally, saying constantly to his wife that he must murder her, and making several attempts to do so. He was transferred to Farnham House, September 5, 1862. His state at that time indicated chronic cerebral disease. His head was relatively hotter than the rest of his body, his pulse slightly accelerated; and although there was no appearance of paralysis, his manner of speaking approximated somewhat to that condition. He talked extravagantly of his wealth, his power, and greatness; said he was emperor of all the world; spoke of his coal mines which produced coals worth £500 a-ton; his marble house; his precious stones, &c. These things he repeated again and again at short intervals, and to every one who came to see him. He spoke to strangers as to old acquaintances, and seemed to have forgotten his most intimate friends.

Unpromising as the case seemed, and thinking it was quite possible that there might be some latent taint about him, he was put upon minute doses of Plummer's pill, combined with quinine and henbane—one to be taken each night. These were not taken regularly, for it was not every night he could be induced to take them. They appeared, however, so far to have agreed that the temperature of the head was certainly reduced, though it still remained hotter than natural. This treatment was begun October 16th, 1862; on the morning of the 21st October, five days after it was commenced, his look was observed to be somewhat altered, and he appeared heavy. He eat his dinner as usual, of chicken and bacon, and got a glass of wine after it. This was about half-past 3 o'clock p.m. At half-past 6 o'clock he was seized with a severe epileptic fit; he became deadly pale in the face, frothed at the mouth, his eyes were strongly twisted in the sockets, the pupils dilated, the limbs convulsed rigidly, the surface of the body was warm, but the soles of the feet rather cold. No cause could be assigned for the attack—

the bowels having been freely opened the day preceding the attack, and no change in his condition otherwise having occurred. His appearance at the time was most ghastly. Between the first seizure and eight o'clock he had, in all, seven severe fits; another came at nine o'clock, when some leeches were applied to his temple—(other remedies, including a turpentine enema, having been previously employed); and another during the period of their application. The next day he was much relieved, was quite conscious, knew his wife, could speak freely; his head was still hot, his feet warm, his bowels freely open, but his mental condition as before the attack.

I determined now to push the mercurial; and, in addition to giving him his pill three times a-day, had a scruple of mercurial ointment rubbed in night and morning for several days. He had another and last fit on the 25th of October. His mouth is now slightly under the influence of mercury, the inunction having been stopped since, and his pill reduced to twice a-day. His condition is, on the whole, satisfactory. His pulse is 87, full and soft—his head is scarcely, if at all, hotter than the rest of his body; the secretions are properly performed, there is no paralysis, and his delirium seems to have undergone a remarkable change. He talks much less extravagantly, is quieter in his manner, and less restless at night; but he is still evidently insane. What the issue may yet be remains to be seen; but I think it highly advisable to push the remedy fairly, and to keep the patient's system under it for a time, supporting his strength, as may be necessary, by tonics, good nourishment, and wine.

With regard to the use of mercury in idiopathic epilepsy, I am far from thinking that the effects are such as to justify its ordinary employment. Except for some special complication, it appears to me to be rather prejudicial than otherwise, diminishing the tone of the muscular system, increasing irritability, and depressing the spirits and vigour of the patient. Hence the diagnosis of these cases is always a matter of importance. In the presence of any obvious signs of venereal action—particularly in an active form—no hesitation need be felt as to the propriety of using it with care and becoming precautions; or even when these are absent, whenever the previous history of the patient suggests a probable ground for supposing that such a condition may exist in a latent form; but, under every other circumstance, I think a cautious practitioner would do well to hesitate lest he only aggravate the complaint instead of relieving it. I have said nothing of the use of hydriodate

of potash in these cases—not because I have any doubt as to the propriety and necessity of using it, but because it appears to me to hold rather the second place, in the treatment, to mercury. In the first two of the cases related it was freely used in addition, and, I have no doubt, with advantage; and it is most probable that in due time it will also be employed in the third. But in the crisis of these alarming cases the palm must, in my judgment, be conceded to the older and more energetic remedy.

Comparing these three cases together, and assuming that we have a right to consider the third as an instance of syphilitic insanity, they illustrate the different ways in which the venereal poison acts upon the nervous system. In the first it produced epilepsy, differing in no respect from the ordinary form of the disease, to be succeeded, after years of suffering, by symptoms of mental derangement; in the second, epilepsy did not occur at all; and in the third, insanity took the precedence, and epilepsy followed. With respect to the form of mental derangement presented by these cases, no two of them appear similar. The first was clearly an instance of wild delirium, as transient as it was violent. The other two had a certain resemblance to each other in the acts of extravagance committed, in the style of their ideas and conversation, and in their general quiescent temper. But here the similarity ends. The one exhibited a tendency to violence—to break furniture, to injure, and even murder, his wife—while in the other no such symptoms could be observed.

Art. IV.—*A Note on Reflex Disease of the Eye, of Traumatic Origin.* By J. Vose Solomon, F.R.C.S., Surgeon to the Birmingham and Midland Eye Hospital.

The Mechanical Irritation of an Orbit from which the Globe has been removed is capable of Producing Symptoms of Reflex Disorder in the Previously Healthy Eye of the opposite side.

W. V., a healthy man, 36 years of age, by trade an iron moulder, received a severe burn of his *left* eye, from the spurting in of some molten iron, on the 1st of July, 1857. He obtained prompt and careful treatment, yet, the vision was lost, and a connecting band

formed between the globe and its lower lid. Early in the following September the right eye suffered sympathetically. It was painful, and there was a sensation of external compression. The vision, which was best in bright light, became, at the same time, disturbed by what appeared to be a cobweb, which constantly floated before the eye. The pain and dulness of sight increased so much that, on the 17th of March of the following year, 1858, the patient requested me to remove the disorganized globe, which was accordingly done by enucleation, in the usual way. The optic nerve was divided close to the globe. In three hours after the operation there was a marked mitigation of the severe cutting pains which had affected the right eye during the last three or four months, and the cobweb (*musca*) appeared to be much smaller. In the course of a short time the band which connected the lower lid to the globe was regenerated, and presented an obstacle to the introduction of an artificial eye. It was therefore divided, and a glass mask inserted in the orbit. The mask excited a good deal of local pain and conjunctival inflammation, which, in its turn reproduced sympathetic ocular disorder of the same degree and kind as had existed before the excision was performed. On the withdrawal of the mask, and subsidence of the conjunctival irritation, the left eye assumed its former condition of usefulness. At the end of a fortnight the mask was once more used, when the sympathetic symptoms reappeared—flashes in the dark (*photopsy*) being superadded. The patient has now for more than four years been engaged in his trade, as an iron moulder, taking the precaution of ceasing work whenever the eye becomes dim or affected with flashes. The sight is generally clear, and he is capable of reading small print without fatigue.

English authors on ophthalmology have hitherto attributed the occurrence of sympathetic traumatic ophthalmia to the commisural arrangement of the optic nerve fibres.

The phenomena presented by the preceding case do not appear to afford a *locus standi* for such theory, inasmuch as the optic nerve on the side of irritation had been severed; but they may be satisfactorily explained on the supposition that the irritation from the eyeless orbital cavity was conveyed by the ophthalmic division of the fifth pair of nerves to the centre, and thence reflected to the opposite side. This view derives support from examples of reflex traumatic disorder wherein the symptoms are purely those of hyperesthesia of the fifth, and which yield, almost *instantaneously*, on removal of the

original source of irritation. Of this class of cases some account is given in a paper which was read by me, before the Birmingham Branch of the British Medical Association, on April 9th, 1857, and published in the journal of that society for June 13th (p. 496) of the same year.

Careful observation of a large number of instances of reflex ophthalmia—a disease of great frequency in Birmingham, and the neighbouring mining districts—has led me to the following, among other conclusions, namely:—that the *primary* traumatic irritation is conveyed to the sound eye through branches of the fifth nerve; that soon afterwards, and sometimes simultaneously, the vaso-motor nerves take on disordered action, and, as a consequence, intraocular congestions and their results occur; that, if the disease be not arrested, the optic nerve fibres become inflamed, or affected with such other changes as induce atrophy, and which lead to similar degeneration of the opposite nerve, attended by an amaurosis, which is incurable. In this stage of the complaint the commisural arrangement of the optic nerve fibres is probably the channel through which the disorder is conveyed.

When the state of the eye admits of its fundus being examined by the ophthalmoscope, in the early stage of reflex ophthalmia, the most constant appearance is congestion of the vessels of the retina, and more especially of the veins; but where there is an advanced amaurosis the usual signs of atrophy of the optic nerve entrance, which are sufficiently familiar to render description in this place needless, are revealed.

Art. V.—*A Case in which a Disease, like Measles, arose from an unusual cause; with some brief remarks.*[a] By Henry Kennedy, A.B., M.B., one of the Physicians in Ordinary to Sir P. Dun's Hospital.

The following case, though but a solitary one, I wish to bring under notice; as it presented some points of curious interest, or even importance. I should state that, at the time of its occurrence, some five months since, I was in total ignorance of its nature. But thanks to our periodical literature, a paper has, since then, come

[a] Read before the Med. Association of the King and Queen's College of Physicians.

under my notice, which has helped to remove the difficulty and make light where darkness had existed. There will be occasion to speak of this paper further on. The case was briefly as follows:—

A young gentleman of fifteen years of age, rather under-sized, but of a high order of intelligence, returned to school after the summer holidays, being then in perfect health. As he entered the school-room one of his play-mates met him, holding a paper bag, with some kind of powder in it, in his hand, and before he was aware, had dashed a handful of the powder in his face; and there can be no doubt that some of it got not only into his eyes, but down his throat—for he was laughing at the moment. The powder turned out to be flaxseed-meal, which, by some accident, the other boy had found in the room.

The result was truly remarkable; the boy was at once seized with smarting and watering of the eyes, running from the nose, cough, and dyspnea. With some difficulty he made his way home —a distance of an English mile. By the time he reached it his face had become much swollen, the eyelids and eyes very red, and the dyspnea urgent. The excitement, too, of the system generally was very great; and all this within two hours of the accident. When seen the following day he had, except the rash, all the look of a boy suffering from a sharp attack of measles. His face was still swollen, his eyes were injected, and had a strange dark-red line round them, giving a very peculiar expression to the countenance; and he had a constant loud cough, with dyspnea. His pulse was 120. Two years previously I had attended him in a well-marked attack of measles, with cough.

On hearing the history of the case, I confess I thought that quiet and a little time would suffice to get him well; and so he was only directed to inhale the steam of boiling water; and, as his distress was referred mainly to the larynx, a small mustard poultice was directed to be applied over that organ night and morning. In this expectation, however, I was much disappointed; and finally, after waiting a few days, when a considerable amount of general bronchitis had supervened, I was compelled to treat the case as if it were ordinary measles, by salines, including tartar emetic, and blisters; and by the end of three weeks, and not till then, could he be pronounced well. The last symptom which remained was dyspnea; for this he got small doses of the oxide of zinc with markedly good effect. The boy is now perfectly well.

This case I set down at the time of its occurrence as anomalous—

for I could scarcely believe that such symptoms would follow such an accident; but, on the other hand, the accurate history given me, and the direct connexion between the accident and the immediate supervention of the symptoms, could barely leave a doubt that the two stood to each other as cause and effect. And so matters remained, and the case had got quite well, when the paper already referred to came under my notice. This paper will be found in the *American Medical Journal* for July, 1862, its title being—"Remarks on Fungi, with an Account of Experiments, showing the Influence of the Fungi of Wheat Straw on the Human System; and some Observations which point to them as the Probable Source of 'Camp Measles,' and perhaps of Measles generally. By J. H. Salisbury, M.D., of Newark, Ohio." Some of the cases given in this paper had been transferred to the *Dublin Medical Press*, and there caught my eye. Its general tenor is to show that soldiers are very liable to a form of measles when obliged to sleep on straw which is old or musty; and two striking examples are given where large numbers of men were so attacked, measles not being in the camp at the time. The author then goes on to state that he treated straw in such a way that it became mouldy within 48 hours; in other words, fungi, of which accurate drawings are given, were produced. With these fungi he then inoculated some 13 persons, the result being that, within two days, a disease exactly like measles in some, including the rash, followed.

These experiments appear to me to prove conclusively that the fungi of musty straw are capable of generating a disease which it seems next to impossible to distinguish from measles. It may be well to give one or two of the cases, condensed for the sake of brevity:—

"Case.—At 10 o'clock, p.m., February 11th, 1862, I inoculated my arm with the spores of the fungi of wheat straw. The straw was the same kind as that used for beds at the camp.

"Wednesday 12th.—Perfectly well; no inflammation at the point of inoculation.

"13th.—Very slight redness and itching at inoculated point.

"14th.—Got up with a feeling of lassitude and nausea; redness and itching on the increase; had difficulty in keeping warm: occasional sneezing; eyes sensitive; a peculiar feeling about the scalp.

"Saturday 15th.—Lassitude, sneezing, &c., continue, with flashes of heat over the whole body; inflammation of wound on the increase: the peculiar burning congested feeling over the scalp has also in-

creased, with pains through the forehead and temples; a few blotches have made their appearance on the face and neck; eyes weak and inflamed; a heavy oppressive feeling about the chest; and the throat dry and irritated, as if I had a severe cold."

It is enough to say, that all the symptoms detailed continued with considerable intensity till Wednesday, 19th February, when the writer found himself much better. He then again inoculated himself, but with no result whatever. The next case he gives is that of his own wife, who, on the second day after the inoculation, began to suffer from severe cold, causing hoarseness and great constriction of the throat, with very considerable fever, sneezing, and sore eyes; also a slight rash visible. These symptoms lasted a week, and subsided gradually; the attack having passed downwards, as it were, to the stomach and bowels.

There are also cases given in the paper where this artificial inoculation, as it may well be called, seemed to act as a prophylactic, by saving children from the ordinary measles, which disease was in the house at the time. But this point is only alluded to here to show the extent to which the author has carried his investigations. It is now very many years since Darwin, if I recollect right, proposed that the tears of those affected with measles should be used for a similar purpose.

To the facts detailed in this paper the case which came under my own notice is now to be added; and, no matter in what aspect it is viewed, it seems to me to open up a question of the greatest interest and extent. For if there be other agents than the fungi of wheat straw capable of generating a disease like measles, where will such powers end. All vegetable and animal substances, too, while in a state of decay, are known to become mouldy—producing fungi or spores; and to such an extent does this go, that even living beings, including ourselves, when in bad health, almost constantly produce them. The revelations of the microscope are, in this point of view, truly startling; showing us that diphtheria, and some of the most inveterate diseases of the scalp have the closest connexion with cryptogamic growths. The potato and vine-blights, too, come exactly within the same category; many other examples might be enumerated; and I would refer any one particularly interested in the subject to a paper published in *M'Millan's Magazine*, for October, 1862. True it is, that in all these instances the fungi are the result, not the cause, of disease. But what I now wish to call attention to is the great universality

of these bodies, whether of animal or vegetable origin, coupled to the fact, which may be considered as absolutely established, that some of them, at least, are capable, when applied to the human frame, of generating disease. And if we farther reflect on the great facility with which these bodies may gain access to our frames, either in our food or the air we breathe, we have a source of disease opened up of which the importance can scarcely be over-estimated. For a long period the opinion has prevailed that epidemics are caused by some such bodies as these. But proof was wanted in support of this idea; and here now we have gained the first step. For it must be repeated again, the experiments of Dr. Salisbury are conclusive:—That certain bodies which are being constantly generated in vegetable matter, are capable of causing certain diseases when inoculated into the human frame. Nay, further, that inoculation is not essentially necessary; and on this point the case detailed by myself seems to be most important, for it shows that the mere contact of fungi with the mucous membranes is sufficient to cause disease, and of a very severe character.[a] The case also proves that other fungi than those of wheaten straw are capable of inducing an affection very like measles. This last point opens out a field of the very widest for investigation. Thus measles presents itself to our notice under different forms. We have this disease with and without cough; a distinction long recognised,

[a] Some may think that the case given by myself may be explained in a different way; that the symptoms may have been caused by the mechanical effects of the meal, and arose too soon to attribute them to the effects of fungi. This view of the case may, of course, be held; but the following reasons induce me to keep to that stated in the text:—It was clearly established that the meal thrown in the patient's face was mouldy; and since then, my friend Dr. Kidd, the present editor of this Journal, has succeeded in bringing meal of the same kind into a similar state, and, by means of the microscope, detecting in it fungi very like, if not identical with, some of those figured in the plate of Dr. Salisbury. These fungi Dr. Kidd showed myself; so that the fact is absolutely established, that fungi may have existed in the meal used by the boy in the school. But then, it will be said, the effects of the fungi arose too soon, for the symptoms were immediate. This I think may be questioned. There is certainly no comparison whatever between the effects of inoculation on the skin and on a mucous surface. If a poison, like putrid pus, be applied to the eye, will it not begin to act at once; and so of the mouth, nose, and stomach. I have seen a sore throat begin at once, from inspecting the same in another. Are not all the stronger poisons, such as corrosive sublimate, arsenic, &c., instantaneous in their action, provided the dose be large and the stomach empty. Nor is it to be forgotten, that in the case detailed, the dose, if I may so speak of it, was large, and applied to the eyes, nose, and mouth at the same moment. For these reasons, then, I consider this case an example of a disease, like measles, being caused by the application of the fungi which are produced in flaxseed-meal which had become mouldy.

and, I believe, correctly. Again, at some periods the throat is very much engaged, and at others quite free; and I myself have seen measles rendered most serious by its complication with acute dysentery. May not such differences, or more correctly varieties, arise from different fungi? One kind prevailing at one period, and a different kind at another. Or may not the way by which the fungi get into the system cause a difference in the results? Thus, in the case I have just detailed, the eyes, mouth, fauces, and wind-pipe were naturally the parts which suffered—the irritation thence spreading to the lungs. But if, in place of this mode of ingress, some of the meal had been at once taken into the stomach, what might not then have been the consequence? Would it have been dysentery or other intestinal irritation? Though, with our present knowledge, these questions cannot be answered, they yet seem to me all-important; and, no doubt of it, the time will come when they will be cleared up, and so a great step gained in practical medicine.

Other considerations arise out of the foregoing statements. Thus they afford a reasonable solution of the cause of certain diseases—as, for instance, the hay-fever or asthma; and may not the affection known as influenza come into the same category? The strange effects, too, of certain substances in the form of powder, of which ipecacuanha is a good example, may result from the same agency; and when smells or odours are of such a kind as to create fainting, nausea, or even vomiting, are there not good grounds now for supposing these may be due to certain particles floating about in the air, and necessarily taken into our frames?

Nor, in a practical point of view, are the preceding facts of less moment; and the following deductions may, I think, be fairly drawn from them:—

1. That certain acute diseases affecting the throat and air passages may be caused either by inoculation of certain vegetable fungi or by direct contact of the same with the mucous membranes.

2. That, as far as is yet known, the diseases so produced seem to have the closest resemblance to measles.

3. That the vegetable fungi which have been long admitted to exist in certain chronic diseases, as those of the skin, show an impaired state of the constitution; and hence the importance of combining a constitutional with a local treatment in their management.

4. That when vegetable fungi cause disease by coming in contact

with the mucous membranes of the head and chest, we have now fixed data for the administration of emetics; which, by their direct effects, may thus cut short disease in its early stage.

P.S.—A second paper, by Dr. Salisbury, has, I find, been published in the *American Journal* for October, 1862. It is also a very valuable communication, as showing, and in a very clear way, the prophylactic power which the inoculation of straw fungi exercises over the human frame, even when exposed to the contagion of ordinary measles. But as this part of the subject does not come within the scope of my present remarks, I can only refer my readers to the paper itself for the details, which are of high interest.

Art. VI.—*On the Phenomena of Diabetes Mellitus.* By the Rev. Samuel Haughton, M.D., F.R.S., Fellow of Trinity College, Dublin.

Part II.

(*Continued from* Vol. xxxii., p. 277.)

In the former part of this paper[a] I gave an account of three cases of diabetes mellitus, in which I attempted to ascertain the excretion, both of sugar and of urea, and from which I deduced certain conclusions, to which, of course, the same value cannot be attached as to the observations themselves.

I shall now add three more cases of this disease, from which the same general conclusions may be inferred:—

For the clinical observations on these cases I am indebted to A. W. Foot, M.B.; and to Doctors Stokes and Hudson, Physicians to the Meath Hospital, for permission to make my observations, and for many valuable suggestions during their progress. I made, myself, all the determinations of sugar and urea in the laboratory of Trinity College.

Case IV.—Thomas Cooke, aged 40, admitted into the Meath Hospital October 1st, 1861, under Dr. Hudson.

[a] D. Q. J. M. S., Vol. xxxi., p. 317, and Vol. xxxii., p. 265.

History of Case.—Is a labourer, and comes from Co. Leitrim; married, of temperate habits, and excellent previous health; no history of hereditary predisposition.

Jan. 1860.—"Got a cough;" spat blood; in latter end of July, 1861, cough returned, and he spat blood, and became affected with great thirst and diuresis; "at the commencement of this cold his urine was not clear;" but three weeks before admission he passed a quantity of urine, estimated at three quarts, clear and sweet (teste linguâ); the quantity of urine now voided is much less, and of deeper colour, "because he stints himself in drink."

Since July his appetite has been bad, his strength and flesh rapidly diminishing.

Condition on Admission.—Passing saccharine urine, sp. gr. 1042; no albumen; of clear gold colour; thirst excessive.

Heart.—Situation, impulse, sounds, normal; pulse, 91, regular.

Lungs.—Percussion clear, before and behind, over each lung; and respiration healthy, except under right sterno-clavicular articulation, where it is rougher than natural; bowels habitually costive; tongue dry, red at tip and sides; posteriorly and in centre a dry brown fur; sordes collect on teeth and lips; "bad taste in mouth continually;" gums spongy and bleeding; appetite bad; skin generally dry, but perspires occasionally; peculiarly greasy between shoulders, and on back generally; has remarked so himself; sleeps well.

Clinical Observations.—On the 8th of October it was discovered that there was fluid effused in the cavity of the peritoneum; the superficial epigastric veins on each side were enlarged and prominent; defecation was attended with pain in the lumbar region, and retraction of the testicles, and the spleen was found to be enlarged; the veins of the left leg and thigh were varicose; copious perspiration comes on while he is straining at stool.

October 9th.—Œdema appeared in both legs, but subsided in a few days; reappeared in right leg on 31st.

November 2nd.—He showed an anthrax forming on outside of right thigh, which, on the 5th, was opened by crucial incision down to the fascia, and bled freely; diarrhea began at this time, and appetite commenced to fail.

November 12th.—Retention of urine required use of catheter, and about 30 oz. were drawn off from the bladder; this operation

was performed also on 13th, 14th, and 15th; the effusion into the peritoneum became increased.

November 18th.—Diarrhea occurs every night; bowels moved every hour; can pass urine without catheter, but with much exertion; suffers much from pain in loins; the diarrhea was very rebellious to treatment; he was subject to occasional profuse perspirations, especially when at stool.

His appetite was very variable, but not at all excessive, nor by any means equal to the usual appetite of this disease. Sometimes he ate his allowance of bread, sometimes half of it, or none.

The following observations were made during his stay in hospital:—

(*a*) 2nd, 3rd October, 1861.—Weight (naked), 146 lbs.; passed 110 oz. fl. of urine, of sp. gr. = 1039·08; sugar = 4010 grs.; urea = 722 grs.

Previous Daily Food.

Bread, 8 oz., . . .	134 grs. urea,	. 2377 grs.	sugar.
Milk, 1 pt., . . .	58 ,,	. 177 ,,	
Tea, 1 pt., . . .	5 ,,	. x ,,	
Porter, 1 pt., . . .	27 ,,	. 15 ,,	
Oatmeal Broth, 1 qt.,	46 ,,	—	
	270 grs. urea.	**2569** grs.	sugar.

(*b*) 8th, 9th October.—Passed 70 oz. fl. of urine, of sp. gr. = 1042·78; sugar = 2553 grs.; urea = 674 grs.

Previous daily food same as before.

(*c*) 15th, 16th October.—Weight = 145 lbs.; passed 90 oz. fl. of urine, rancid in smell; sugar = 2461 grs.; urea = 709 grs.

Previous Daily Food.

Bread, 12 oz., . . .	200 grs. urea,	. 3566 grs.	sugar.
Mutton, 8 oz., . . .	368 ,,	—	
Milk, 1 pt., . . .	58 ,,	. 177 ,,	
Tea, 1 pt., ...	5 ,,	. x ,,	
Eggs, 2, ...	92 ,,	—	
Porter, 1 pt., . . .	27 ,,	. 15 ,,	
Oatmeal Broth, 1 qt.,	46 ,,	—	
	796 grs. urea.	**3758** grs.	sugar.

(*d*) 23rd, 24th October.—Passed 64 oz. fl. of alkaline urine; sugar = 1666 grs.; urea = 896 grs. Previous food same as before.

(*e*) 29th, 30th October.—Weight = 145 lbs.; passed 68 oz. fl. of alkaline urine; sugar = 1750 grs.; urea = 714 grs.; phosphoric acid (combined with earths = 16·06 grs., with alkalies = 22·73 grs.); total = 38·79 grs. Previous food same as before.

(*f*) 5th, 6th November.—Passed 51 oz. fl.; sugar = 1115 grs.; urea = 558 grs. Appetite fallen off.

A large anthrax, which had formed on the back of the right thigh, was opened this morning (6th November), and bled freely; I took the opportunity to collect the blood for the purpose of examining it carefully for sugar. One ounce of this blood was evaporated to dryness at 212° F., reduced to powder, and percolated with alcohol (of sp. gr. 0·828), which did not dissolve the dextrine; I examined it for sugar with the cupro-potassic solution, and weighed the oxide of copper, which I found to be 0·95 grs.

As one ounce of sulphate of copper is used to make 12630 grs. of this solution, of which 1000 grs. are equivalent to 5 grs. of glucose; we readily find the following chain:—

	1 oz. blood.	
100	95 grs.	oxide of copper.
40	125 grs.	sulphate of copper.
7000	16 oz.	,,
1	12630 grs.	standard solution.
1000	5 grs.	glucose.
	0·428 grs.	glucose.

From this it is easy to calculate that there is 0·98 of a grain of glucose in every 1000 grs. of blood. At the time when the blood contained this quantity of sugar, 51 oz. fl. of urine contained 1115 grs. of sugar, or 21·863 grs. per ounce; dividing this by 0·428, the quantity of sugar in the ounce of blood, we find the fraction $\frac{1}{51}$th, to represent the dilution of sugar in the blood, as compared with that in the urine. I found, in the case of Owen Murphy, a theoretical dilution of $\frac{1}{42}$nd, from the measurement of the aorta and renal arteries; and it must be admitted that the present experiment falls in with the filter theory of the action of the kidneys.

(*g*) 13th, 14th November.—The catheter had to be used during the day; by means of it 40 oz. fl. were drawn off, which contained, sugar=1000 grs.; urea=682 grs. I obtained this day $1\frac{1}{4}$ oz. of blood by cupping, which I boiled and washed on a filter, with boiling water, passing the droppings into strong spirit, to coagulate the dextrine; this I afterwards collected on a weighed filter, and

found it to weigh 0·30 grains. From this it may be inferred that 1000 grs. of blood contained 0·548 of a grain of dextrine.

(*h*) 20th, 21st November.—Passed 40 oz. fl. of urine, sugar = 795 grs.; urea = 560 grs.

Shortly after this period Cooke left the hospital, at the desire of his friends, who wished him to settle his affairs, and returned to the country (Co. Leitrim); I have not been able to ascertain what became of him.

Mr. Foot has supplied me with the following table:—

Thomas Cooke.

	Urine		Weight (naked)
1861	Oz.	Sp. gr.	lbs.
Oct. 2, 3	110	—	146
„ 13, 14	70	1040	—
„ 14, 15	100	1039	—
„ 15, 16	90	1042	145
„ 16, 17	80	1045	—
„ 17, 18	60	—	—
„ 18, 19	80	1038	—
„ 21, 22	75	1040	—
„ 22, 23	68	—	143·5
„ 23, 24	64	1045	—
„ 24, 25	38	1045	—
„ 25, 26	88	1040	—
„ 27, 28	70	1047	—
„ 28, 29	55	1045	—
„ 29, 30	68	1045	145

Thomas Cooke—con.

	Urine		Weight (naked)
1861	Oz.	Sp. Gr.	lbs.
Oct. 30, 31	58	1043	—
Oct. 31, Nov. 1	95	—	—
Nov. 3, 4	80	1043	—
„ 4, 5	40	1045	—
„ 5, 6	51	1043	—
„ 6, 7	40	1045	—
„ 7, 8	58	1040	—
„ 11, 12	60	1040	—
„ 12, 13	30	1040	—
„ 13, 14	44	1043	—
„ 14, 15	48	1043	—
„ 17, 18	30	1042	—

Collecting together the preceding results, we obtain the following table:—

Table XIII.—*Case of Thomas Cooke.*

	Date, 1861	Weight	Urine	Sugar secreted	Sugar ingested	Urea excreted	Urea ingested
a	3rd Oct.	146 lbs.	110 oz. fl.	4010 grs.	2569 grs.	722 grs.	270 grs.
b	9th Oct.	—	70 oz. fl.	2553 grs.	2569 grs.	674 grs.	270 grs.
c	16th Oct.	145 lbs.	90 oz. fl.	2461 grs.	3758 grs.	709 grs.	796 grs.
d	24th Oct.	144 lbs.	64 oz. fl.	1666 grs.	3758 grs.	896 grs.	796 grs.
e	30th Oct.	145 lbs.	68 oz. fl.	1750 grs.	—	714 grs.	—
f	6th Nov.	—	51 oz. fl.	1115 grs.	—	558 grs.	—
g	14th Nov.	—	40 oz. fl.	1000 grs.	—	682 grs.	—
h	21st Nov.	—	40 oz. fl.	795 grs.	—	560 grs.	—

In the preceding table it is remarkable that the excretion of both sugar and urea, on Cooke's admission to hospital, was much in excess of the corresponding quantities ingested; showing that the change from his former diet to the hospital diet did not produce immediate effects. In all the experiments, except the first (*a*), the diet described lasted for the whole week preceding. When his appetite failed, the diet taken could not be well estimated, as it varied from day to day. He presented at this period, and subsequently, symptoms of enlarged liver and spleen, with some dropsical effusion into the peritoneal cavity; which latter symptom, I have no doubt, was connected with the diminished excretion of urea and urine.

Case V.—The next case that I shall describe is that of a young man, named John Murphy, in whom the predisposing cause of the disease seemed to have been the practice of Onanism; he was under the care of Dr. Stokes, who concurs with me as to the probable cause of his complaint.

John Murphy, aged 25, admitted into Meath Hospital December, 1861.

Has had this disease four months; the canine appetite exists now two months; thirst was the first symptom; has a morning cough and dry skin.

The following observations were made during his residence in the Meath Hospital:—

(*a*) 13th, 14th December, 1861.—Passed 200 oz. fl. of urine; sugar = 7292 grs.; urea = 919 grs.

Previous Diet.

Bread, 1 lb., . . .	267 grs. urea,	4755 grs. sugar.
Beef, ½ lb., . . .	368 ,,	—
Broth, 1 qt., . . .	46 ,,	—
New Milk, 1 qt., .	116 ,,	354 ,,
	797 grs. urea.	**5109** grs. sugar.

With 3 oz. fl. brandy, and 2 grs. of opium.

(*b*) 20th, 21st December.—Weight = 136 lbs.; passed 273 oz. fl. urine; sugar = 9953 grs.; urea = 1075 grs.

Previous diet same as before.

(*c*) 8th, 9th January, 1862.—Weight = 134 lbs.; urine = 471 oz.; sugar = 13737 grs.; urea = 1277 grs.

Previous diet same as before, with addition of 6 oz. of bread, equivalent to 100 grs. of urea, and 1783 grs. of sugar; making a total of 897 grs. urea, and 6892 grs. sugar.

(*d*) 16th, 17th January.—Weight = 133 lbs.; urine = 277 oz.; sugar = 12119 grs.; urea = 970 grs.

Previous diet same as (*a*) and (*b*); urea = 797 grs.; sugar = 5109 grs.

(*e*) 23rd, 24th January.—Weight = 136 lbs.; urine = 233 oz. fl.; sugar = 8321 grs.; urea = 866 grs.

Previous diet same as (*c*); making urea = 897 grs., and sugar = 6892 grs.

(*f*) 30th, 31st January.—Weight = 137 lbs.; urine 220 oz. fl.; sugar = 7549 grs.; urea = 866 grs.

Diet same as before, with two eggs; making urea = 989 grs.; sugar = 6892 grs.

(*g*) 6th, 7th February.—Weight = 136 lbs.; urine = 230 oz. fl.; sugar = 8385 grs.; urea = 1006 grs.

Diet same as last.

(*h*) 13, 14th February.—Weight = 140 lbs.; urine = 230 oz. fl.; sugar = 8527 grs.; urea = 755 grs.

Diet same as last, with one ounce of brandy added.

(*i*) 20th, 21st February.—Weight = 143 lbs.; urine = 240 oz. fl.; sugar = 9545 grs.; urea = 997 grs.

Diet same as last.

(*j*) 6th, 7th March.—Weight = 145 lbs.; urine = 240 oz. fl.; sugar = 9130 grs.; urea = 892 grs.

Diet same as last.

(*k*) 13th, 14th March.—Weight = 142 lbs.; urine = 220 oz. fl.; sugar = 7700 grs.; urea = 866 grs.

Diet same as last.

(*l*) 27th, 28th March.—Urine = 190 oz. fl.; sugar = 7557 grs.

Diet same as last

Collecting together the preceding observations into one table, as before, I find the following:—

TABLE XIV.—*Case of John Murphy.*

	Date	Weight	Urine	Sugar excreted	Sugar ingested	Urea excreted	Urea ingested
a	1861. 14th Dec.	—	200 oz. fl.	7292 grs.	5109 grs.	919 grs.	797 grs.
b	21st Dec.	136 lbs.	273 oz. fl.	7953 grs.	5109 grs.	1075 grs.	797 grs.
c	1852. 9th Jan.	134 lbs.	471 oz. fl.	13737 grs.	6892 grs.	1277 grs.	897 grs.
d	17th Jan.	133 lbs.	277 oz. fl.	12119 grs.	5109 grs.	970 grs.	797 grs.
e	24th Jan.	136 lbs.	233 oz. fl.	8321 grs.	6892 grs.	866 grs.	897 grs.
f	31st Jan.	137 lbs.	220 oz. fl.	7549 grs.	6892 grs.	866 grs.	989 grs.
g	9th Feb.	136 lbs.	230 oz. fl.	8385 grs.	6892 grs.	1006 grs.	989 grs.
h	14th Feb.	140 lbs.	230 oz. fl.	8527 grs.	6892 grs.	755 grs.	989 grs.
i	21st Feb.	143 lbs.	240 oz. fl.	9545 grs.	6892 grs.	997 grs.	989 grs.
j	7th Mar.	145 lbs.	240 oz. fl.	9130 grs.	6892 grs.	892 grs.	989 grs.
k	14thMar.	142 lbs.	220 oz. fl.	7700 grs.	6892 grs.	866 grs.	989 grs.
l	28thMar.	—	190 oz. fl.	7557 grs.	6892 grs.	—	989 grs.

The weights in these tables are only correct within a pound avoirdupois, as they were not taken with the scales, but by means of a spring balance.

If we take from the preceding table the observations (*e*) to (*k*), inclusive, during which the dynamical equilibrium was established, we find the following results:—

1. The sugar ingested per day was 6892 grs.
2. The sugar excreted per day was 8451 „
3. The urea ingested per day was 976 „
4. The urea excreted per day was 893 „

There is a deficiency here of 1559 grs. of sugar, which it appears to me impossible to account for, except on the supposition that the proteinic compounds were resolved into sugar as well as into urea. In this transformation no carbonic acid is given out; on the contrary, if the view I have advanced—*D. Q. J. M. S.*, Vol. xxxii., p. 269—be correct, a small quantity of carbonic acid is retained.

The following appears to me to be a correct view to take of this case. Assuming the weight at 140 lbs., and the minimum excretion of urea at 2 grs. per pound, we find:—

1. Urea excreted, ...	893 grs.
2. Minimum for opus vitale, ...	280 „
3. Urea produced with glucose from proteinic compounds,	613 grs.
4. Corresponding glucose produced from proteinic compounds,	3065 grs.
5. Glucose ingested, ...	6892 „
	9957 grs.
6. Glucose excreted, ...	8451 „
Excess to be accounted for, . . .	**1506** grs.

If this be converted into carbonic acid, and excreted by the lungs, it will give:—

7. Carbonic acid formed from glucose, . . .	2008 grs.
8. Carbonic acid formed with urea of opus vitale,	1643 „
Total carbonic acid, ...	**3651** grs.

This is a quantity of carbonic acid less than half of what is excreted in health by a person of 140 lbs. body-weight. Unless, therefore, the deficiency were made up by the combustion of fat, which is quite possible, we should be forced to the conclusion that the excretion of carbonic acid fell far below its natural amount in health.

I made several, but quite ineffectual, attempts to estimate the amount of carbonic acid excreted per day, by the method of Dumas, and others; but came to the conclusion that no result as to quantity is of much value, unless it involve the total amount excreted in 24 hours. This important result has been recently attained by Pettenkofer and Voit, at Munich; and I would venture to suggest to these distinguished physicians the importance of placing a diabetic patient in their celebrated air-chamber for 24 hours, in order to determine with accuracy his excretion of carbonic acid. I feel satisfied that the result would show a serious deficiency, as compared with the excretion of health.

Case VI.—Owen Butler,[a] aged 50, the father of six children,

[a] His brother died of same disease after one and a half-year's illness.

admitted into Meath Hospital August 1st, 1861; at 12 months before admission the symptoms of this disease appeared; and were attributed by himself to his occupation of threshing, which he has followed for 23 years, working at it from morning till night; has lost flesh, and strength, and eye-sight has grown dim since his illness; hunger is very great; sensation of a tearing in his stomach, and a pain in his forehead, are felt when his appetite is not satisfied; has a craving for fat, and drinks cod-liver oil with avidity. Has found milk to assuage thirst best.

On the occasion of his first visit to hospital the following measurements of his urine were made:—

1861,	Aug.	5th, 6th,	390 oz.—24 hours,		1042 sp. gr.
„	„	18th, 19th,	260 „	„	1043 „
„	„	21st, 22nd,	270 „	„	1040 „
„	Sept.	3rd, 4th,	205 „	„	1041 „

He perspires at night, often profusely; has no pulmonary symptoms.

Aug. 5th, ordered Pulv. Dov. gr. x, ter die.
„ 16th, „ „ „ quinque die.
Sept. 5th, „ „ „ sex die.

Aug. 19th, weight (naked) 131 lbs.

Sept. 9th, weight 128 lbs. On this day he left hospital in a fit of depression, saying he would go home to die. He has since[a] three times enjoyed and left the hospital, and is soon to return for a fifth visit. When he began to take 40 grs. daily of Dover's Powder he was "heavy and drowsy," and often vomited; but after the dose was increased to 50 grs. per diem he did not vomit, but got drowsy for a short time after each powder, and "started wonderfully in his sleep at night."

Second Visit.—Admitted again in Nov., 1861.

Nov.	28th, 29th,	160 oz. urine,	1047 sp. gr.	131 lbs. wt.
Dec.	13th, 14th,	188 „	1043 „	131 „

Third Visit.—Again in hospital; much emaciated, and feebler than on last visit; weighing 117 lbs. (naked); has pains and coldness in legs and thighs; feels as if his lower extremities were beaten with nettles; œdema sometimes about ankles; bowels moved every second day.

Observations.

1862, July	27th, 28th,	150 oz. urine,	1043 sp. gr.	117 lbs. wt.
„ Aug.	11th, 12th,	205 „	1042 „	120 „

[a] Written in January, 1863.

Admitted for fourth time, October 25th, 1862. Sight very much impaired; appetite failing; is now little more than ordinary hunger; thick mist before his eyes; legs perfectly cold from knees down; takes violent fits of shivering; sleep broken by starts and involuntary jerkings of muscles; gums soft, frequently bleed; has stings of pain along the back, and cramps in his thighs when wakening from sleep; has creeping and tickling sensations in nerves of both upper extremities. Brandy makes him sleep comfortably at night. Skin now always dry and coarse (used to perspire on first visit).

Observations.

Oct. 27th, weight 118 lbs. (naked).
Nov. 10th, ,, 117 ,, ,,

Left Hospital, December, 1862.

During the time that Butler was under my observation I made the following notes of his case:—

(*a*) 28th, 29th November, 1861.—Weight=131 lbs.; urine=160 oz. fl.; sugar=5000 grs.; urea=1050 grs.

Previous Food.

Bread, 1 lb.,	267 grs. urea,	4755 grs. sugar.
Beef, ½ lb.,	368 ,,	— ,,
Broth, 1 qt.,	46 ,,	— ,,
Milk, 1 pt.,	58 ,,	177 ,,
Porter, 1 pt.,	27 ,,	15 ,,
Tea, 1 pt.,	— ,,	— ,,
	766 grs. urea.	**4947** grs. sugar.

(*b*) 13th, 14th December.—Weight=131 lbs.; urine=188 oz. fl.; sugar=6854 grs.; urea=864 grs.

Previous food, same as last, with addition of pint of milk and pint of porter; making urea=851 grs; sugar=5139 grs.; 3 oz. fl. of brandy and 2 grs. opium also added.

(*c*) 20th, 21st December.—Weight=129 lbs.; urine=229 oz. fl.; sugar=7156 grs.; urea=1002 grs.

Previous diet same as last.

(*d*) 8th, 9th January, 1862.—Weight=129 lbs.; urine=180 oz. fl.; sugar=6300 grs.; urea=1102 grs.

Previous diet same as last, with addition of six ounces of bread, making the ingesta—

Urea=951 grs.; sugar=6922 grs.

(*e*) 16th, 17th January.—Weight=126 lbs.; urine=160 oz. fl.; sugar=6363 grs.; urea=966 grs.

Previous diet same as last.

(*f*) 23rd, 24th January.—Weight=128 lbs.; urine=205 oz. fl.; sugar=6523 grs.; urea=1076 grs.

Previous diet same as last.

(*g*) 30th, 31st January.—Weight=128 lbs.; urine=188 oz. fl.; sugar=6854 grs.; urea=1110 grs.

Previous diet same as before, with the addition of two eggs, making the ingesta—

Urea=1043 grs.; sugar=6922 grs.

(*h*) 13th, 14th February.—Weight=129 lbs.; urine=204 oz. fl.; sugar=7285 grs.; urea=1071 grs.

Previous diet same as last.

Collecting together, as before, these results into one table, we find:—

Table XV. *Case of Owen Butler.*

	Date	Weight	Urine	Sugar excreted	Sugar ingested	Urea excreted	Urea ingested
a	1861. 29th Nov.	131 lbs.	160 oz. fl.	5000 grs.	4947 grs.	1050 grs.	766 grs.
b	14th Dec.	131 lbs.	188 oz. fl.	6854 grs.	5139 grs.	864 grs.	851 grs.
c	21st Dec.	129 lbs.	229 oz. fl.	7156 grs.	5139 grs.	1002 grs.	851 grs.
d	1862. 9th Jan.	129 lbs.	180 oz. fl.	6300 grs.	6922 grs.	1102 grs.	951 grs.
e	17th Jan.	126 lbs.	160 oz. fl.	6363 grs.	6922 grs.	966 grs.	951 grs.
f	24th Jan.	128 lbs.	205 oz. fl.	6523 grs.	6922 grs.	1076 grs.	951 grs.
g	31st Jan.	128 lbs.	188 oz. fl.	6854 grs.	6922 grs.	1110 grs.	1043 grs.
h	14th Feb.	129 lbs.	204 oz. fl.	7285 grs.	6922 grs.	1071 grs.	1043 grs.
	Means,	129 lbs.	189·2 oz. fl.	6542 grs.	6229 grs.	1030 grs.	926 grs.

This case presents some features strikingly resembling that of Owen Murphy, particularly as respects the almost complete equality of the sugar excreted and ingested. This, however, I regard as an accidental circumstance, as the excretion considerably exceeded the ingestion in the cases of M'Nee and John Murphy.

Assuming the equality of this ingestion and excretion in Butler's case, we have to account for the excretion of carbonic acid by the decomposition of the proteinic food. This must take place either

by the natural action of health, equation (4), (*D. Q. J. M. S.*, Vol. xxxii., p. 272), or by that of the disease which I have supposed in equation (1), (Vol. xxxii., p. 269); in either case it is insufficient to produce the carbonic acid required.

1. On the supposition of healthy production of carbonic acid, the urea excreted, 1030 grs., would develop only 6043 grs., whereas the lowest minimum of health requires 9100 grs. of carbonic acid.

2. If the proteinic compounds produce both urea and glucose, it can be shown that in this case also the carbonic acid is deficient.

We have on this supposition—

1. Sugar ingested, ...	6229 grs.
2. Sugar abnormally formed with 770 grs [1030 minus 260] of urea, ...	3850 grs.
Total, ...	10,079 grs.
3. Deduct sugar excreted, ...	6542 grs.
	3537 grs.
4. Carbonic acid produced from last found sugar, ...	4716 grs.
5. Add carbonic acid produced as in health, with 260 grs. of urea, min. op. vit., . . .	1525 grs.
	6241 grs.
6. From this we must subtract 565 grs. (equivalent to 770 grs. urea), retained, ...	565 grs.
Total carbonic acid, ...	**5676** grs.

Whichever view, therefore, we take of the question, there appears to be in diabetes mellitus a deficiency in the excretion of carbonic acid.

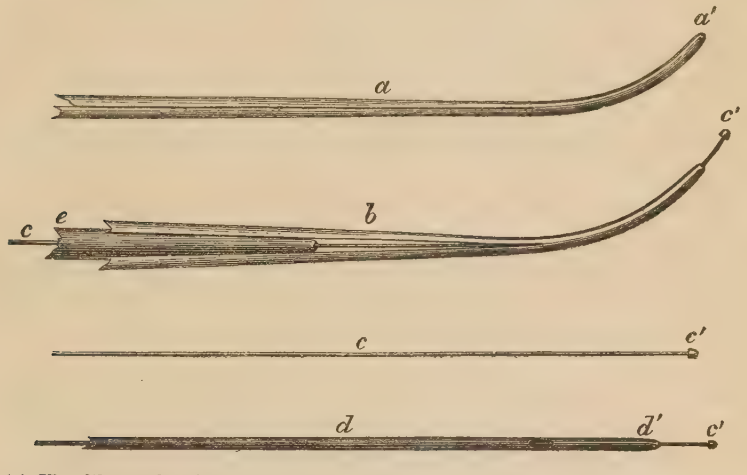
I greatly regret my inability to confirm or disprove this conjecture, which is not without practical interest from its connexion with the theory of phthisis, and I hope others who have the means will not neglect to try the experiment.

Art. VII.—*On the Treatment of Stricture of the Urethra by the more "Immediate Plan."* By Philip Crampton Smyly, M.B., one of the Surgeons to the Meath Hospital.

In the November number of this Journal, page 297, there is a very able essay on Mr. Bernard Holt's "Immediate Plan," by Prof. Macnamara. I refer to this paper, and also to Mr. Holt's own very interesting book, in order to call the reader's attention to the fact, that in almost every case a certain amount of dilatation was necessary, before the dilator could be introduced. This is indeed self-evident, as the dilator itself is equivalent to No. 3, and many instruments I have seen are so large as No. 4 catheter. In the case of T. N., p. 303, in Mr. Macnamara's paper, I passed No. ½ for several days before I could get in No. 1, and several more before I could increase the size to No. 3. Even when dilated so far there was much difficulty in introducing the metallic dilator. After the dilator was once in the bladder, nothing could be more satisfactory than the conclusion of the operation, and the whole course of the case. I have since, on more than one occasion, passed No. 11 into this man's bladder without the slightest difficulty. Mr. Holt does not mention, in all his cases, whether he employed this preliminary dilatation or not. In case No. 1 he evidently did not. He says (page 13):—"After the lapse of a third week, and while the patient was fully under the influence of opium, a third attempt was made, but with no better success. I therefore determined to place the patient thoroughly under the influence of chloroform, when, after very considerable difficulty, a No. 1 catheter was introduced; great constitutional disturbance followed, so that a month elapsed before any further instrumental attempt was made. Former experience having shown the improbability of introducing any instrument, excepting while under chloroform, he was once more anesthesiated, and the dilator, with difficulty, passed into the bladder." In Mr. B. Holt's hands such a proceeding may be without much danger. But we all know that there are such things as false passages, and also that a very good surgeon *may* make one. And though, as Mr. Holt says, "the operation is of the most simple kind, and any one who can pass a bougie through a *difficult* stricture is competent to perform it," I would ask the young surgeon—or even a somewhat experienced surgeon—if he would not have some hesitation in bursting a stricture with No. 10 or 12 tube, through which he had

forced a metallic instrument, equal in size to No. 3 or 4 silver catheter, the stricture being so tight as only to admit No. 1?

I do not make these remarks in any way to disparage Mr. Holt's plan. My object is simply to call attention to an instrument I have devised, which combines the advantages of Mr. Holt's plan and of the method employed by Mr. Hutton of this city, which is familiarly called the "railway catheter," and has been still further developed, recently, by Mr. Wakley.



(*a*) The dilator closed.

(*b*) The branches slightly opened to show the wire guide in the position of the stilette in Holt's instrument. The dilator (*e*) is not quite pushed to the end.

(*c*) The guide of silver wire.

(*d*) No. 1 catheter, either of silver or gum elastic, to which any curvature may be given.

The instrument consists of a No. 1 catheter (*d*), or even smaller, having an orifice at the point. The guide (*c*) is about twice the length of the catheter, and is terminated at the vesical end by a small silver button (*c'*), which exactly fits the orifice of the catheter (*d'*). There is no ring at the opposite end of the guide, so that the catheter may be withdrawn from the bladder without removing the guide.

The dilator (*a*) is, with a few exceptions, similar to Mr. Holt's. The differences are:—1st. There is no stilette; 2nd. The point is perforated (*a'*) to fit the guide I have described above; and 3rd. The point of the instrument is made conical. The shafts are grooved exactly like Holt's, and the arrangements about the handle are identical.

The mode of using this instrument is sufficiently simple. The catheter, armed with the guide, is passed through the stricture into the bladder. The catheter is then withdrawn over the guide which remains in the urethra; the catheter (*d*) is partially withdrawn to show the guide. The dilator is substituted for the catheter, as Wakley would substitute a catheter of a higher number. It is pushed down through the stricture until stopped by the button (*c'*) at the end of the guide in the bladder. The instrument is now exactly Mr. Holt's, and the further steps of the operation, namely, the bursting the stricture, may be completed by passing down any of Mr. Holt's tubes on the guide. I should further remark that the catheter may be either of gum-elastic or of silver.

The advantages of this combination appear to me to be the following:—

1st. The stricture may be dilated at the first passing of an instrument.

2nd. That, with patience and gentleness, a stricture may be passed with No. 1, in very inexperienced hands, where even the most talented surgeon, even with chloroform, would fail to introduce No. 3.

3rd. That a gum-elastic catheter, with a fine stilette, may often be passed into the bladder when it is very difficult or impossible to pass a metallic instrument of the same number.

4th. That, whether the instrument first introduced be metallic or elastic, a certain guide is left behind for the dilator, which can then and there only be forced through a tight stricture without fear.

And 5th. I may add that as the instrument does not require to be so strong as Mr. Holt's, it can be made much smaller in size.

PART II.

REVIEWS AND BIBLIOGRAPHICAL NOTICES.

Epilepsy; its Symptoms, Treatment, and Relation to other Chronic Convulsive Diseases. By J. Russell Reynolds, M.D., Lond., F.R.C.P., Assist. Phys. University College Hosp., &c., &c. London: Churchill. 1861.

It is very creditable to modern medicine that so much labour and patience should have been, during the last few years, expended upon a subject comparatively so ungrateful as epilepsy. But since the publication of Dr. Marshall Hall's work on the neck as a medical region, the contributions to the literature of convulsive diseases have been singularly rich and numerous. The labours of our best abstract pathologists and practical physicians have alike been devoted to the unveiling the obscure origin of these affections, the tracing their remote influences, and the endeavouring to control them by remedies. We need but mention the names of Bernard, Brown-Séquard, Hoffman, Kussmaul, Tenner, Van der Kolk, and of Todd, Winslow, Radcliffe, Sieveking, Richardson, and Reynolds, to instance both the earnest search after scientific truth that so characterizes the foremost of medical workers of the present decade, and the interest attaching to the subject so exceptionally complicated and obscure. The present volume is the last of a series that has materially furthered the progress of theoretic medicine. Dr. Radcliffe's, as is well known, deals with the subject from a theoretical point of view. His notion, that muscular contraction is to be ascribed to withdrawal of nervous influence, and that spasm and convulsion depend, of necessity, therefore, upon defect in nerve power, although foreshadowed by the Germans, is yet sufficiently original to attract for it a very large share of attention. Dr. Sieveking presented us with a compendium which, coming as it did from an observer of so enlarged an experience,

possessed features recommending themselves particularly to the practitioner. Dr. Reynolds avoids the originality of the one, and the didactic style of the other, and furnishes us with a work which is both learned and interesting—impartial, full, practical, and philosophic.

There is one evil attending a "run" on a special subject—an evil which has displayed itself conspicuously in recent medical writings, and which has, in fact, led to the quasi-recognition, under the generic term epilepsy, of many conditions and diseases to which epilepsy is but very remotely allied. The name by which the disease is familiar is appended, in the adjectival form, to distinguish certain other diseases having nothing in common with the former but a certain subjective similarity. Thus, the late Dr. Todd believed gout and diabetes to be very general disorders, and, accordingly, he distinguished, by some facility peculiarly his own, a gouty kidney and a diabetic pneumonia. Similarly he had his epileptiform seizures, his renal epilepsy, and his epileptic coma; and this phraseology—tending to destroy classification, to create a false nosology, and a forgetfulness of idiopathic disease—is now so common that its legitimacy is but seldom suspected. It is in this way that convulsive diseases, from having been termed epileptiform, have got to be thought epilepsy in fact. They may be simply convulsive movements; they may be transient losses of consciousness, or both combined in an undecided form; yet the term epilepsy is indiscriminately applied to all, notwithstanding the origin of them being, for example, in other organs than the nervous—as in the blood, or in the membranes of the encephalon. The disease epilepsy can, in fact, scarcely be said, of modern date, to have distinctive features, origin, or course. It has been disguised under the several forms—centric, eccentric, diathetic, and toxemic, according as such conditions simulating the symptoms of epilepsy proper may have been present, either associated respectively with, or deducing their apparent origin from, disease of the nerve centres, disease of remote parts, or a poisoned state of the circulating fluids.

Dr. Reynolds treats only of simple and idiopathic epilepsy. He takes eighty-eight cases in which he could discover no trace of any other affection than the very remote one to which the symptoms collectively termed epilepsy should be referred. And his plan obviates the discrepancies to which the statements of authors equally able and conscientious are alike liable. If one group of convulsive cases be meningitic, another cataleptic, another diathetic, it is evident

no accurate conclusions can be drawn from observations upon them, undertaken with the view of investigating epilepsy *per se.* Epilepsy may be traced to heart disease, for example. But heart disease is a frequent precursor of dropsy. If, then, it be not observed that the epilepsy is not idiopathic, it might be concluded that dropsy and epilepsy stand to each other in some obscure and interesting relationship. Symptoms cannot, in fact, be taken as a basis of investigation, any more than inflammation can be taken as a groundwork on which to found a distinction between, for example, cancerous and tuberculous infiltrations, either of which may give rise to an inflamed condition.

In most diseases symptoms indicate not simply a morbid condition either of structure or function, but a condition whose primary seat is readily fixed by the indications pathological research places at our disposal, and whose nature may almost be inferred by a recognition of the seat of the morbid process. Sudden and acute pain in the side, with dyspnea, speaks of an inflammation in the lung, or its investing membrane; and pathology reveals at once to us such conditions as naturally bring about the very symptoms that engage the attention. Puffiness under the eyes, with casts and albumen in the urine, lead unmistakably to a recognition of such changes in the secreting structure of the kidney as guide at once to diagnosis, to prognosis, and to treatment. But the nature of the symptoms—epilepsy—the cry, the loss of consciousness, the convulsions—are themselves as obscure as the cause that brings them about.

And an additional difficulty besets our subjective study of this curious disease. These symptoms are not only in themselves obscure, but of inconstant recurrence and simultaneity. Were the cry always heard on the occurrence of an epileptic seizure, we might, we think, have some little ground for concluding that the locus of the disease was in the reflective centre; or were the convulsions accompanied in all cases by entire loss of consciousness, a localization of the starting point to such portions of the cerebrum as would include the hemispheres might reasonably be made. But the cry is often absent. The convulsions are witnessed independent, occasionally, of marked loss of volition; and, doubtless, in the majority of cases, the loss of consciousness occurs independently of either apparent convulsion or spasm.

In considering the symptoms further, we obtain little beyond uncertainty of the nature of the lesion, even though we seize upon the typical form of seizure, standing, as it were, between the one

extreme of disturbed balance of mind, and the other of paroxysms of convulsions without marked loss of consciousness. Such seizure manifests a loss of consciousness, with involuntary muscular actions. But even here these latter phenomena are essentially different in different cases. In the one case there may be local tonic spasmodic movements; in the other, general tonic and clonic convulsions; distinctions which may, in practice, be useful to decide between *le haut mal et le petit mal*, but which evidence, it must be confessed, the almost hopeless character of the pathology of the subject.

Following upon the researches of Dr. Reynolds, we find, in the class of cases undoubtedly epilepsy, the widest possible differences in the muscular spasms in successive attacks. At one time the muscles of the eye only are set in action; at another, and in no observed relationship to the amount of stupor, the entire body is rendered rigid by the universality of the contraction. A special proclivity of certain sets of muscles has been thought to be observed by some observers—the frequency with which epileptics utter expiratory noises pointing to the muscles of respiration. At other times the muscles of deglutition are mainly concerned; and in others, so frequently is trachelismus observed, that, as is very well known, Dr. Marshall Hall considered this symptom capable of affording a most important insight into the pathology of the disease. But Dr. Reynolds inclines to think that this connexion of trachelismus and epilepsy is by no means so intimate as Dr. Hall supposed. Most of the difficulties in the way of forming a rational hypothesis of the nature of the central lesion may, however, be met, as Dr. Reynolds observes, by the assumption, that in epilepsy the loss of consciousness is invariably associated with, and dependent upon, contraction, the result of motor impulse, but that this impulse may be confined to the contractile fibres of the cerebral vessels. The element of spasm need not, in fact, occur of necessity in muscles exposed to observation.

Given, then, the symptoms of the disease, what is the organ wherein these have originated? It was constantly observed that persons dead of epilepsy presented, on *post-mortem* examination, thickening of the cranial bones, calcareous depositions in their fibrous lining, dilatation of the capillaries in the medulla oblongata, structural alterations and enlargement of the pituitary body and pineal gland. By the simple process of induction not a few held to the belief that these changes—effects, in fact, of the disease—were

in reality so many of its causes. But repeated observations have at length fully proved that such lesions, curious as they are, were not only but a few of those which epileptic subjects present, but that they might themselves be absent altogether.

As much interest was created by Wenzel's observations on the morbid conditions of the pituitary body, as has originated, in our own time, in the discovery of the effect of irritation on the fourth ventricle; and not a little remarkable is the fact that, when once the former body was supposed to be affected, enlargement, with alteration of its contents, were never found to be absent. Wenzel made twenty autopsies, of epileptics, from 1804 to 1808, and always found these pathological indications. What a prize for the inductive school. The most subtle of diseases at length found to be also due to anatomical change! Unfortunately the pituitary body, something like the supra-renal capsules of to-day, began to evidence a not inseparable connexion with the disease they were supposed to produce, and the observations of Rokitansky and Romberg set the theory, as is well known, adrift altogether.

The view of most notoriety that has been propounded by pathologists, next to that respecting the pituitary body—viz., the view of MM. Bouchet and Cazauvieilh—has not, any more than that of Wenzel, been confirmed by experience; and as it has been found that the conditions they observed were not constant, and indeed that epilepsy may occur when the cerebral lobes have been removed—that is, so much of epilepsy as is constituted by convulsions—chronic inflammation of the white cerebral substance can, at best, be considered as a possibly concurrent phenomenon.

It was not one of the least merits of the late lamented Van der Kolk, that, earnest believer as he was in positive pathology, he avoided the error of his school, and hesitated to believe that the alterations in the calibre of the capillaries of the medulla oblongata were any other than proximate results. Doubtless, if these vessels are enlarged, or if granular degeneration be present in a given subject, epilepsy is not unlikely to result as an effect; but neither state holds as a constant element—as an "essential organic cause;" and the true value of his investigation consists, not in the discovery of the changes themselves, but in the observation of the site in which these and like morbid conditions are found to be localized.

In his *Physiologie du Systeme Nerveux*, Georget referred the disease exclusively to the brain. The deduction, apparently simple, was yet one of the highest value, inasmuch, as once conceded, it

restricted the observations of his followers to this organ. Prochaska laid down in his *Memoirs on the Reflex Functions of the Medulla Oblongata and Medulla Spinalis*, quoted by Dr. Reynolds, that the sensorium commune acts independently of consciousness in producing the convulsive movements of epileptics. Van der Kolk advanced a stage beyond this, and deduced, from the observations before mentioned, his belief that the starting point of the various convulsive movements in epilepsy is to be found in the medulla oblongata. The experiments of Marshall Hall, Weber, and Todd have most materially tended to fortify this view of the locality of the immediate starting point of the irritation. That convulsive movements originated in disturbance at the spinal centre, and not in the cerebrum, was shown by Marshall Hall; and while galvanization of the cord itself was found, by Weber, to produce tetanic spasms, while galvanization of the medulla oblongata induced spasmodic phenomena in a clonic form, Dr. Todd's well-known experiments sufficiently demonstrated the share of the mesocephalon and quadrigeminal bodies in bringing about epileptiform convulsions, results, as it is now familiar knowledge, that have been fully confirmed by Kussmaul, and Tenner, and Brown-Séquard.

The locus penitentiæ being then fairly assumed, what is the nature of, to use the familiar expression, the irritation that sets out from it? We have convulsions and loss of consciousness, be it remembered. The former is the manifest result of irritation in the medulla, the latter of change in the cerebral hemispheres. Can we trace any mutual interdependence of these? Have we to deal with an anemic or a congestive irritation.

The former question can safely be answered in the affirmative. Irritation propagated along the vaso-motor nerves, from medulla to cerebrum, or *vice versa*. Irritation producing spasm, spasm diminution of calibre in the affected vessels, consequent arrest of circulation and loss of consciousness. But need this spasm be transmitted at all? Does the loss of consciousness stand in fact, though not apparently, in second order of sequence? In other words, does the manifest change in the medulla always precede that in the cerebrum, and yet be always so minute at the onset that its existence becomes only manifest by means of the increased irritation, sufficient to produce spasm, reflected from the centre, whose nutrition it itself disturbed To confuse us, rather than to guide us to a solution, we have, on the one hand, the difficulty the instantaneous kind of loss of consciousness creates; and, on the other, those minute precursory

muscular movements which indicate, prior to loss of consciousness, a disturbed condition in the functions of the reflective centre.

Congestion has been the favourite condition assumed to constitute the change in which the perversion of function, wherever primarily occurring, consists. For this Marshall Hall argued, more especially in his *Memoirs on the Neck;* and upon the presence of congestion, Dr. Bennett explained not only the loss of consciousness, but the convulsions. We do but follow Dr. Reynolds in remarking that the recognition of congestion, as a primary fact in the pathology of the attack, dates from the time of Hippocrates. With him but few, since the experiments of Kussmaul and Tenner, will be disinclined to agree that such congestion is not only not a primary fact in epilepsy, but that, at the onset of the attack, there is no evidence of it, and that it is not the cause of loss of consciousness. Assume the existence of irritation, and it will not justify the supposition of congestion ensuing. Contraction of the arteries of the pia mater has been shown, by Donders and Callenfels, to follow upon irritation of the sympathetics; and Brown-Séquard has demonstrated that the arteries of the brain meninges contract through reflex stimulation, whose centre is the medulla oblongata. If to these results we couple the observations that Kussmaul and Tenner adduce—that compression of the principal arteries of the neck will produce loss of consciousness and epileptic seizures—we have every reason to conclude that the irritation is brought about by a condition most certainly not one of congestion.

Indeed, by what may be termed synthetical reasoning, this matter has been, in fact, almost set at rest through the converse experiments of Kussmaul and Tenner directed to the investigation of the effects of an induced plethora of the brain. Tying the external and internal jugular veins of some rabbits, they divided the cervical branches of the sympathetic. The respirations diminished from 135 to 18 a minute, the breathing became snoring, and the glottis paralyzed. The animals did not lose consciousness or the power of sitting upright; stupefaction became marked and slight; transient convulsive movements at length succeeded; but all these symptoms disappeared as gradually as they had come on, without the animals having fallen into real epileptic convulsions.

These experiments accord very forcibly with the observation of Romberg—that the epileptic convulsions and coma in plethoric subjects are of a markedly apoplectic character. Indeed it may reasonably be doubted whether the occurrence of such and analo-

gous symptoms in plethoric persons can be termed epileptic. Should they not rather be held to belong to the apoplectic class, characterizing affections possessing in common, unconsciousness, insensibility, and slight clonic spasmodic movements of the limbs, but differing in so far as the state of the respiratory muscles is concerned, and being, in particular, accompanied by paralysis of the glottis, whilst epilepsy is distinguished by spasm of the glottis. Epileptic attacks can certainly be produced by spasms, confined, at the commencement, to the muscles of the vessels solely; although the experiments in proof are beset with many difficulties; for the ascending cervical branches of the sympathetic operated upon do not seem to be the only sources of nervous supply to the carotids and their branches.

It might, we think, be modestly observed, in objection to these views, so learnedly discussed by Dr. Reynolds, based as they are upon the experiments of the illustrious physiologists we have named, that the very nature of the loss of consciousness consists in its suddenness, which, traceable to such a cause, can but indicate what must amount to complete occlusion occurring at the central point from which these vessels arise, in the part where the vaso-motory nerves take their origin; and if not there, the arrest of circulation would take place only at such a distance as involves the necessity of admitting the establishment of collateral circulation. Nor is it amiss to point out the fact that, if the theory be correct in all its details, a very considerable support is obtained for the hypothesis of Dr. Radcliffe, that muscular action is mainly due to the abstraction of nervous force. In what then does the nature of the morbid change really depend? Clearly not of necessity in structural alteration. The interstitial processes of which nutrition consists, and upon which function depends, must be altered in quantity, or quality, or both. The disturbance, then, says Dr. Reynolds, is functional.

"It is the proper function of this portion of the nervous system to transmit impressions, both motor and sensory, and to reflect certain afferent impressions, through efferent nerves to muscles. Now, the essential elements of a convulsive paroxysm exist more or less frequently during health, and as a part of the healthy processes of the body. The unconsciousness of sleep is a normal phenomenon; the continuance of involuntary and even irresistible muscular movements is equally compatible with, and necessary, to normal life. The manner in which the former is brought about is not accurately determined, but the most

probable mode is that suggested by Dr. Laycock, viz., that the 'access of affinitive impressions is arrested;' and that this depends upon 'such changes in the medulla oblongata that the changes in the sensory nerves . . . . are not propagated to the organs of thought and will.'[a] On the other hand, under the influence not only of the will but of emotion, sensation, and simply reflex impression, there are occuring hourly various muscular movements and changes in the superficial circulation. Respiration has its centre in the medulla oblongata; and, easy and tranquil as it is when no source of disturbance is at hand, it becomes suspirious or sobbing under emotion; yawning and irregular under fatigue; and positively arrested by fear, or by the effort required for a strong physical exertion. The teeth and hands are clenched by anger; the face is distorted by grief, flushed by excitement, suffused by shame, and blanched by fear; and thus the phenomena of convulsions, dissected out as it were, occur in our daily life as parts of our healthy activity. There is nothing to show, therefore, that the changes upon which epilepsy depends, are of necessity modifications in the kind of function exercised by the organ from which it starts. Misplaced in time, in combination, and altered in degree, those functions are; but no new property is conferred upon the organ, nor is any natural power changed in the quality of its exercise.

"If altered—but not in kind—the functions of this reflective centre must be changed in degree; and we have to ask whether there is increased or diminished action. The answer is, I think, simple enough, if—setting aside ulterior questions as to the nature and cause of muscular contraction—we inquire what is the proper action of the organ. If it is to put in motion certain trains of muscular contraction, such as those conducing to respiration, emotional expression, and the like, then that action is increased, as well as misdirected at the onset of a fit. For not only is the symmetry of feature distorted, but violent movements of the face-muscles are induced; the eyeball is carried whither no voluntary effort could effect its movement; the tongue is crunched, and the teeth are broken by the forcible champing of the jaws; the neck and head are strained to frightful extremes; the scream of the attack, and the arrest of respiration—now from one cause, such as a closed glottis, and again from another, such as fixed diaphragm and thoracic walls—are of the nature of violent exertion; and the torsion of the limbs and the vehemence of their movements are all of such kind as to demonstrate excess of action.

"In the maintenance of equilibrium, the rest which results from opposed but balanced forces, there is power as active—it may be more so,

[a] Mind and Brain; or, the Correlations of Consciousness and Organization. Vol. ii., p. 301.

although not so conspicuous—as in the tossing to and fro of the same forces when the balance is disturbed. But it is not all equipoise in healthy action, and one-sided action in disease. The ordinary movements of respiration are indications of unrest; the face is not actionless, when the mind and emotions are most healthy, but all its features are in play; there is not unchanging activity of brain during the whole lifetime of the healthy man, for in his waking and his sleeping hours there is the swinging to and fro of the pendulum of intellectual and moral life, now in relation with every impression from the external world, and again cut off therefrom by the mysterious barrier of sleep. In the paroxysm of epilepsy, on the other hand, at its severest moment, there is absolute balance of respiratory movement; muscles of inspiration and expiration are as it were 'pitted against each other,' and stand still with spasmodic energy; facial expression is lost, for it bears no relation to either mind or heart; and, again, the swinging to and fro of mental activity is arrested by an unnatural calm. In all this there is the evidence not only of disturbed equilibrium, but of distorted, misdirected, exaggerated power; and we must regard the paroxysm as the evidence of increased functional activity."

Our readers will here clearly perceive that these are not the views of Dr. Radcliffe. That epilepsy is connected with anything like over-action of the nervous system this latter authority altogether denies. It is only fair to state that Dr. Reynolds has a distinct and special meaning of his own for the term "action." His "action" is of complex character, and partakes of the nature of reaction; for he observes, in the reflex function there is the conversion of an impression from without into a motor impulse from within. In health there is a due proportion between these two factors; in disease it is distorted, and either one may be increased; the balance in the reflective centre is readily disturbed, and this want of permanent functional stability is a constant element. Where there is but little spasm there exists but the simple readiness of disturbance; where convulsions are strongly marked the balance is disturbed, and motor-excitability exalted in addition.

The deduction from this, it is at once seen, is that epilepsy is a sthenic malady. It is a disease produced, says our author, by over-action. Over-action is the exponent of increased functional activity, which is itself the "immediate result of augmentation in the interstitial metamorphosis of organ." To this there is an objection—an objection that naturally flows from an observance of those conditions which we have seen by experiment, and acknowledged in practice, most readily bring about the closest simulation

of epilepsy proper. These conditions are, for example, those of anemia, of blood-poisoning, of general cachexia, or created by lesions inducing shock and prostration; and whether inducing brain disease or exhausting eccentric irritation. And, at least, it must be confessed that this theory of sthenic over-action would derive additional support from an absence of those remarkable facts which clearly show that a morbid state, precisely similar to, and almost identical with epilepsy proper, is capable of being induced under such circumstances as justify inferences altogether antagonistic.

It should be noted, that out of the cases examined by the author there were 30 persons, or 42 per cent., with regard to whom no cause of the attacks could be assigned. From the data afforded him by the remaining cases Dr. Reynolds was able to conclude that the primary change was brought about either hereditarily or by intense functional disturbance from violent external impression; or by the medulla becoming involved in some general nutrition-changes of morbid kind and degree; or changes normal in kind, but of morbid degree; or by a combination of various causative conditions; or by lesions in the reflective nervous centre; or outside that organ; the latter operating either directly or by reflex action.

True epilepsy is a rare disease in the experience of Dr. Reynolds. Out of 1,820 patients seven were examples of the idiopathic disease; and of these four were males, and three females. In private practice 81 patients were afflicted with epilepsy out of 231 cases of nervous disease. There is a remarkable fact about these statistics, that the poorer classes seem to suffer in the proportion only of 1 to 14, while the proportion among the richer is as 1 is to 3. We suspect that this difference is partly to be ascribed to the hurried nature of hospital practice; and perhaps not a little to the fact that the private patient is but too apt to conceal the real nature of his case, or at least to ascribe some reason for it which would place it out of the "idiopathic" list.

The statistics of Dr. Reynolds further show that proclivity to nervous disease from transmission existed in 31 per cent. of his patients; and he concludes that an hereditary tendency to epilepsy is much more common than is usually supposed. M. Delasiauve found evidence of hereditary taint in but nine per cent. Dr. Sieveking makes it 11 per cent Dr. Reynolds has it as much as 16 per cent. From our author's carefully compiled tables it is also apparent that, whereas in the male sex hereditary epilepsy seems especially prone to commence at puberty—six out of seven cases appearing between

15 and 18 years of age—in the female sex there does not appear to be the same proclivity.

From the table noting the conditions which might be held to constitute the causes of the disease it would appear that mental work and emotional disturbance are more commonly operative than any other. The age at which these influences are supposed to have begun to take effect is worthy of attention. The three males who assigned mental work as the cause of their attacks were none older than 15, while the one female, who referred her epilepsy to mental and emotional causes combined, was six years of age!

One frequent cause of epilepsy—although the first attack may be deferred for many years—is, we are satisfied, blows on the head in childhood, most commonly sustained in falling off chairs, and so on. Dr. Forbes Winslow, in his work on *The Obscure Diseases of the Brain and Mind*, states that his experience has led him to assign such causes of epilepsy to be frequent, even among adults. "Repeatedly," he observes, "I have had under my care cases of epilepsy bidding defiance to all treatment, whose origin could unquestionably be traced back—for varying periods of one, two, five, eight, ten, fifteen, and even twenty years—to damage done to the delicate structure of the brain by injuries inflicted mechanically upon the head."

One of the most interesting points connected with the subject of epilepsy is altogether shrouded in obscurity, and this is the amount of constant relationship of the disease to the mental condition. The affection of the mind, at least, indicates this much—that in whatever the essential nature of the disease consists, some of it still persists during the intervals between successive attacks. Indeed this marked disturbance of the higher functions of the brain affords the strongest evidence of the essential seat of the disease; and the determining the laws that regulate this disturbance is at once the most difficult and interesting problem that either the psychologist or physician can propose to himself for solution. Irrespective of hereditary taint or cause, period of invasion, frequency of recurrence, severity, or duration, what is the actual mental condition of the epileptic during the intervals? In most cases there is marked mental disturbance. Esquirol states, that four-fifths of the epileptic women in the Salpetrière were insane. In a considerable number the loss of mental power is, though not remarkable, yet sufficient to excite attention; and indeed it may be doubted whether the powers of mind of any epileptics are altogether unaffected. Dr. Reynolds

separates epileptics into four classes. Among the individuals in the first class he places those who present no apparent change in mental capacity; that is to say, we opine, who let none but themselves know of their own deficiencies in this particular.

In the second class are comprised those sufferers who present defect of memory. We quite agree with Dr. Reynolds that the real nature of this defect is constituted by defective attention.

In the third class the apprehension is defective, the loss of memory is present, as in the former class, and, in addition, these patients are dull in acquiring new ideas, and have very confused impressions of arguments and circumstances.

The fourth class includes those in whom the failure of the mental powers is more or less complete. The patient is, to all intents and purposes, an idiot.

The more developed forms of mental disturbance, though replete with the highest interest, are not such as can engage us here. The more practical question is the determining these conditions, the recognition of which will enable the physician to foresee a seizure, and whose appropriate and well-timed treatment may contribute to the cure of the sufferer.

It is a curious question, how far the exaltation of the mental faculties, which not very seldom signalizes the commencement of epilepsy, can be held to indicate the advent of that exaggerated interstitial change to which Dr. Reynolds refers. Morel, as quoted by Dr. Winslow, states, that among the young a wonderful aptitude to conceive things quickly, to examine them under their most brilliant and poetical aspects, has been exhibited by many of them. History has transmitted to us the names of several men of great genius who have been epileptics; but, as the late Dr. Burrows remarks, these individuals have been the victims of the most tyrannical passions. The full and entire preservation of the faculties of epileptics, the power of applying them in a continuous manner to the execution of designs remarkable for their grandeur and continuity are, in fact, excessively rare. M. Sandras properly points out that Cæsar, Mahomet, and Petrarch, form striking exceptions to this supposed rule.

The first change remarked in the character is said, by Morel, to be excessive irritability and erotic tendencies. Dr. Sieveking remarks, that there is scarcely an impression referrible to the nerves of common or muscular sense, or of the special senses, which does not occasionally indicate the approach of the seizure. Are these

really referred sensations, and has the aura no real eccentric origin? Dr. Winslow observes, that authors commonly state that when the aura reaches the head insensibility ensues; but that it appears mostly the fact that the termination seems to be at the throat.

Impairment of attention is, no doubt, the most common precursory sign of epilepsy; and this fact obtains, in the best works on diseases of the mind, its due share of notice. Dr. Winslow dilates upon this failure simply and graphically:—

"The patient finds that he cannot, without an obvious and painful effort, accomplish his usual mental work, read, or master the contents of a letter, newspaper, or even a page or two of a favourite book. The ideas become restive, and the mind lapses into a flighty condition, exhibiting no capacity for continuity of thought. Fully recognising his impaired and failing energies, the patient repeatedly tries to conquer the defect, and, seizing hold of a book, is resolved not to succumb to his sensations of intellectual incapacity, psychical languor, and cerebral weakness; but he often discovers (when it is too late to grapple with the mischief) that he has lost all power of healthy mental steadiness and normal concentration of thought. In his attempt to comprehend the meaning of the immediate subject under contemplation, he reads, and re-reads, with a determined resolution, and an apparently unflagging energy, certain striking passages and pages of a particular book, but without being able to grasp the simplest chain of thought, or follow successfully an elementary process of reasoning; neither is he in a condition of mind fitting him to comprehend or retain, for many consecutive seconds, the outline of an interesting story, understand a simple calculation of figures, or narrative of facts. The attempt, particularly if it be a *sustained* one, to master and converge the attention to the subject which he is trying to seize, very frequently increases the pre-existing confusion of mind, producing eventually *physical* sensations of brain, lassitude, and headache. Going through a train of close reasoning," he goes on to say in the words of Dr. Beddoes, "is an undertaking absolutely impracticable. Indeed to dwell upon any one thought steadily is a task, and a task, too, that can only be gone through at long intervals. Some person has remarked of a former king of Prussia, that his conceptions were quick, but that on contemplating a subject he grew confused. Whether it be true in this particular instance or not, the observation holds good of many individuals predisposed to epilepsy. They are generally those who have tampered with their sensibility. They seize a question dexterously, but their strength is exhausted in the first assault. If you try to make them grapple with a difficulty they immediately flinch. To any proposition requiring them to contemplate a number of ideas stedfastly, they

will yield a flat unintelligible assent; or, to mask their want of bottom, as the jockeys term it, they will endeavour to fly off to another topic. To conceive the condition of the head in such cases more distinctly, we may recollect how it fares with the eye when weakened in such a manner, that the instant it is cast upon an inscription, the characters are perfectly plain, but that in a little time they seem to run into each other, they become undistinguishable, and at last vanish altogether. From misconduct of the understanding all frivolous people must be troubled with some flightiness of attention. We need no other reason to enable us to understand why it becomes requisite in polite circles to change the topic of conversation every second minute."

The conclusions at which Dr. Reynolds arrives on these points he thus succinctly lays down:—

"1st. That epilepsy does not necessarily involve any mental change.

"2nd. That considerable intellectual impairment exists in some cases; but that it is the exception, and not the rule.

"3rd. That women suffer more frequently and more severely than men.

"4th. That the commonest failure is loss of memory; and that this, if regarded in all degrees, is more frequent than integrity of that faculty.

"5th. That apprehension is more often found preserved than injured.

"6th. That ulterior mental changes are rare.

"7th. That depression of spirits and timidity are common in the male sex, but not in the female; that excitability of temper is found in both."

Dr. Reynolds' chapter on treatment is as well and as logically written as is all the rest of his work, and that is very logically and very well indeed. He ridicules the notion of specifics, and aims at treating the epileptic condition by reducing undue excitability in the reflex centre by sedatives, counter-irritation, and the maintenance of a regimen; and by improving the mental and general health.

Of sedatives, conium, he believes, delays the attacks and diminishes their severity. Hyoscyamus, belladonna, and stramonium diminish their number according to his own experience. He seems to have had no experience of atropine, or of the valerianate of the alkaloid so vaunted by Azario, Lange, and other continental physicians. In one case an habitual series of seventeen severe attacks was reduced to three, by Indian hemp in the usual dose. Cotyledon umbilicus he finds altogether valueless. The effects of assafetida, in doses of gr. x. to gr. xxx., thrice daily, he has found similar to those of henbane.

Chloroform delays the attacks for a time, but exerts no permanently good influence.

Dr. Reynolds has not obtained that success from the employment of bromide of potassium that Sir Charles Locock is stated to have met with. It seems tolerably certain, however, that this medicine is a decided anaphrodisiac. The oxide of zinc seemed to possess most influence in those cases in which vertiginous sensations were frequently accompanied by insomnia. In common with most of those who had the satisfaction of seeing Mr. Holthouse's *castrato*, to whom the nitrate of silver had been freely administered previous to the operative compliance with his eccentric wishes, Dr. Reynolds considers the salt to be nothing less than it is popularly supposed to be—*lapis infernalis.*

We see no mention of the curare, or of the selinum palustre, or of digitalis, as recommended by Duclos; or of the alisma plantago, as prescribed by Hochstetter; or of the Galium album, so highly commended by Dr. Ogle; or indeed of any of the more recent so-called remedies. Perhaps the Doctor credits them with just as many virtues as pertain to the prescription he gives us of Leuens:—

"Take the powder of Osmond, and the root of Peony, and the powder of Moztegan, and drinke all these with stale ale, and let them say their prayers ; and as soon as the party falleth downe, and give the sicke to drinke with good ale that is stale, and by God's grace he shall never have the falling evill any more:—proved."

Considerations Pratiques sur les Hernies Ombilicales Congénitales et leur Traitement. Par le Docteur Debout.

Practical Considerations on Congenital Umbilical Herniæ, and their Treatment. By Dr. Debout. From the Bulletin Gen. de Therapeutique. 8vo, pp. 35.

In this essay Dr. Debout gives a very complete account of a form of disease or malformation which had not hitherto attracted the degree of attention that the frequency of its occurrence or its practical importance ought to have obtained for it.

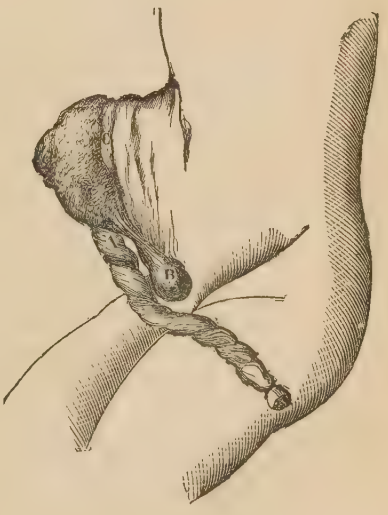
What little has been done concerning congenital umbilical hernia

is rendered obscure by the variety of names that has been adopted by different writers. By some it has been named from the region it occupies; and we have it called *exomphalos*, *omphalocèle*, or *congenital umbilical hernia*. By others it has been named from the contents of the tumour; and so we have such terms as *hépatomphale* and *hépatocèle;* and others again, referring it to an arrest in the development of the abdominal wall, designate it under the name of *ventral hernia* or *congenital umbilical eventration;* but if we were to restrict ourselves to these titles, there are many cases that would escape us which should be included, such as many of those where the cord is ruptured during labour, an accident that may easily occur in cases of umbilical hernia, from the extreme delicacy of the membranes forming the walls of the tumour; and Dr. Debout suggests, with a view to facilitating the study of the subject, that the general title *congenital umbilical hernia* should be adopted, adding to it the denomination of the tumour as a sub-title, such as *hepatocele*, when the liver alone fills the cavity of the hernia, or *eventration*, when the umbilical opening invades the surrounding regions.

Congenital umbilical hernia presents itself under the aspect of a tumour,

Fig. 1.

Fig. 2.



of more or less considerable size, occupying the region from which it draws its name—a tumour with transparent walls, through which the viscera, contained in its cavity, may be seen. In their definition of it most authors contrast the congenital hernia with that which occurs after birth, saying, that in the congenital the tumour is formed, not by viscera which have escaped from the abdomen, but by organs which have never been enclosed; but Dr. Debout thinks this observation, ingenious as it is, only applies to a small number of the cases, and he thinks the peculiar nature of their coverings the most important distinguishing feature of the congenital herniæ.

The transparent walls of a congenital hernia are formed of two distinct membranes, between which there is deposited a layer of the substance known as the *gelatine of Wharton*.[a] The superficial membrane is the external layer of the cord, and the internal one is a membrane completely foreign to the structure of the cord, and appears to Dr. Debout to be formed by a very thin prolongation of the peritoneum; but the remarkable phenomena of which this membrane is the seat, during the spontaneous cure of the lesion, oblige him to express himself on this point less affirmatively than the most of authors do. Diverticula belonging to serous membranes in other malformations—*spina bifida*, for example—do not comport themselves in the same manner.

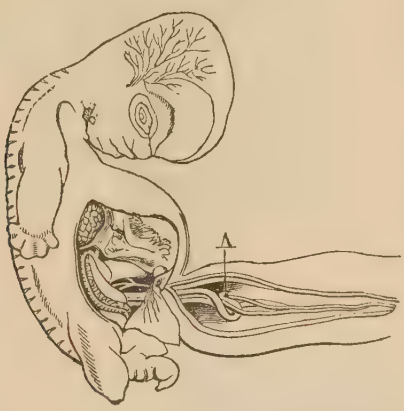
The umbilical vessels are much separated at the base of the tumour by the viscera contained in it, and their direction is not always the same; but at some point of the surface they are re-united, and proceed in their course in the undilated part of cord. The viscera contained in the hernia are, generally, a portion, more or less considerable, of the liver or of the intestines; sometimes the stomach and spleen pass into it. As often a part of the liver alone forms it, and at other times, but more rarely, the intestinal convolutions. The difference of the contents depends on the period of intra-uterine life at which the malformation occurs.

Dr. Debout recognizes two forms of congenital umbilical hernia: 1st, that occurring in the embryonic period, from an arrest of development; and 2nd, that occurring at the end of the fetal period of life. Notwithstanding that Cruveilhier denies that the abdominal viscera are, at any period of embryonic life, situated in the base of the umbilical cord, Dr. Debout maintains that the first species alluded to is actually caused by an arrest of development,

[a] When the gelatine of Wharton is abundant it often forms cysts, as at B, Fig. 2.

and refers to the plates of Hunter, Wrisberg, and Velpeau, and to a preparation of an embryo in his own possession, as well as to several in the museum of Professor Coste, as proving that, at the beginning of intra-uterine life, the cord is a hollow organ, and serves as a diverticulum to the abdominal cavity; and he copies the annexed engraving from *Coste's Atlas of Embryology*, in which a coil of intestine is seen at A in the base of the cord, as exhibiting its position at this period.

Fig. 3.



In the normal state this portion of the intestine retreats into the abdomen, between the seventh and tenth weeks; but if the progress of development be in any way arrested a hernia is the result; and, in some instances, the portion of the intestine in the cord runs through all the stages of its development there, and becomes irreducible. Dr. Debout compares this to fruits which, at an early period, have been placed in bottles, and when they have come to maturity cannot be removed through the opening that had received them, and says, that in the same way the rudiment of intestine contained in the base of the cord continues to develop itself, till it has acquired such a size that it cannot be restored to the abdominal cavity. Such herniæ contain only convolutions of intestine, and these belong to the colon and the end of the small intestine, the portions of the digestive tube primitively contained in the base of the cord. Figure 4 is taken from an example of this form, communicated by Professor Moreau, but in this case the envelopes of the cord gave way at an early period, and the convolutions of the

intestine developed themselves freely in the amniotic fluid, and

Fig. 4.

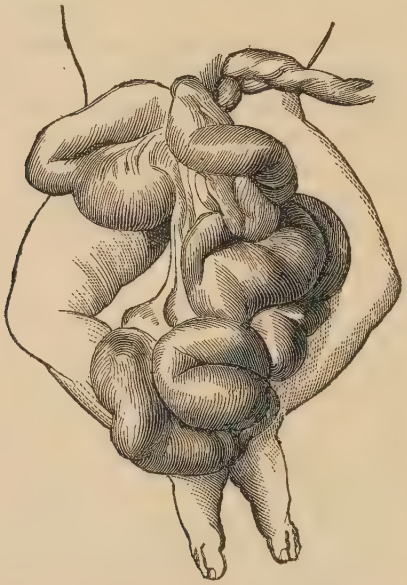
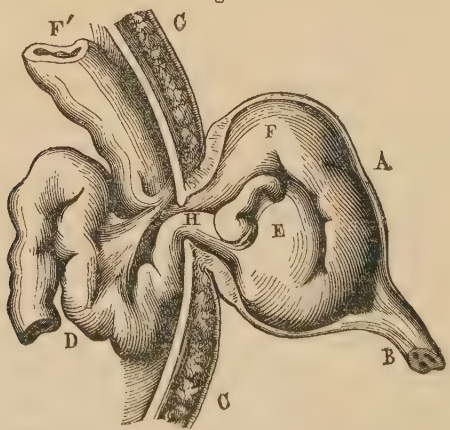


Fig. 5 is from a section made in a case that occurred in the practice

Fig. 5.

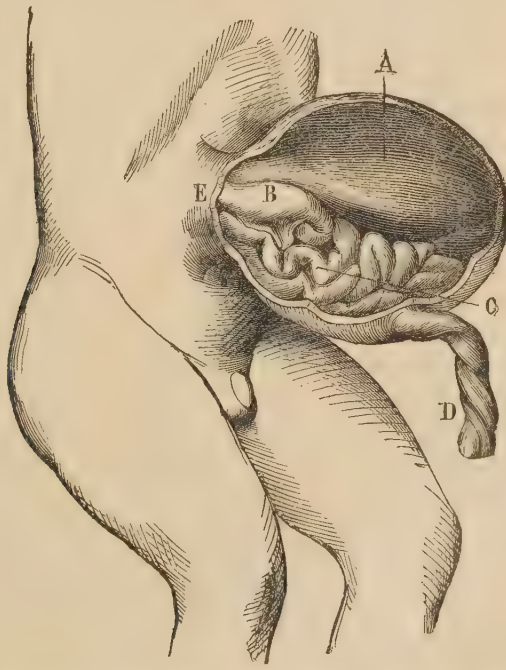


of M. Giraldès, where the portion of intestine in the hernia consisted of the cecum, E, a small portion of the ascending colon, F, and of the small intestine, being the portions of which the rudiment is seen at A, in Fig. 3. In some cases the liver passes through the opening maintained by the intestine, and may be retained in the hernia,

together with the intestine, by its relations with the umbilical vein, and sometimes it even forces the intestine back into the abdomen.

Cruveilhier believes that it is a more satisfactory theory than that of arrest of development, which attributes the hernia to the effect of some compression the fetus has been subjected to, or some vicious attitude it may have assumed, whereby the viscera have been pushed forwards; but Dr. Debout thinks the facts now recorded disprove this explanation of the majority of cases of congenital umbilical hernia, while he admits that it may be true of the cases of hernia occurring at the close of fetal life, and refers to the experiments of Scarpa, as proving that pressure from behind, against the umbilical opening, or traction on the cord, may cause a protrusion of the peritoneum, and form the commencement of a hernia—the tumour, as in the other form, passing into the base of the cord, and being covered by its membranes. Thus he recognizes two forms of congenital umbilical hernia:—1. Those due to an arrest of development, by which the portion of the digestive tube,

Fig. 6.



originally lodged in the base of the cord, does not retreat into the cavity of the abdomen. 2. Those caused by displacement of a

convolution of the intestine contained in the abdomen, and either drawn or pushed against the base of the cord by a compression or vicious attitude of the fetus. Hernia of this description, as in the case from which the illustration, Fig. 6, was taken, may sometimes contain the entire liver and a portion of intestine, and be pediculated; but such cases are of interest only to the teratologists, as there is no instance of a fetus so affected having attained its complete development; they are always expelled before the seventh month of pregnancy.

The nature of the coverings of congenital umbilical hernia renders its diagnosis very easy, if its volume be at all considerable; but when the base of the cord contains only a convolution of intestine it often passes unperceived, and the intestine is included in the ligature applied to the cord, giving rise to all the symptoms of strangulation, and to death if the intestine be completely surrounded; but if only a small part of its circumference be included in the ligature, the child may escape with a stercoraceous fistula.

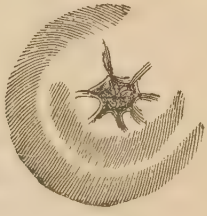
Almost all the older authors think a congenital umbilical hernia incurable. Ruysch says:—"*hunc affectum sœpius a me visum ast nunquam curatum memini; omnes enim ab utero ad tumulum delati fueri*, 5°, 6°, 7°, 8°, 9° *die*." And Amb. Paré relates that, having been called to operate on one of these cases, he refused "et lui dis qu'il *mourrait bien sans moi*." The occurrence of cases of spontaneous cures has forced modern surgeons to appeal from the judgment of their predecessors, and a complete study of the subject shows that the prognosis must vary with the several forms of the tumour.

In the herniæ occurring at the end of fetal life, and which are formed by a small fold of intestine in the root of the cord, the prognosis is favourable if the hernia does not pass unperceived. The intestine is replaced, and the cord tied as usual, and the protrusion of the intestine prevented by the ligature during the closing of the opening. In the herniæ by eventration, if reducible, a spontaneous cure may occur. The external covering of the tumour, which belongs to the cord, separates where it is continuous with the skin, but generally at a later period than the cord usually falls off; then the internal membrane, which is continuous with the muscles and the peritoneum, becomes covered with granulations, and, contracting gradually on itself, reduces the viscera situated in the cavity of the tumour, and finally brings the borders of the opening into contact. For the accomplishment of this process it is

necessary that the hernia should be reducible; and, according to Dr. Debout (but, in this, our experience leads us to differ) the *hépatomphales* are the only forms of this kind of hernia in which this condition exists. In the management of these cases Dr. Debout recommends that we should limit ourselves to retarding the separation of the cord, and favouring the natural process of cure, avoiding all attempts at closing the opening, or bringing its edges into contact. The hernia, he says, should simply be covered with lint saturated with oil, to prevent evaporation, and very moderate pressure kept up by a bandage, with a view to facilitating the enlargement of the abdominal cavity. The integument around the opening is thickened, and forms as it were a cushion, and it is important to prevent its becoming inflamed, as it is by the unfolding of this and its being drawn over the aperature that the cicatrix is formed. In order to prevent its being irritated, Dr Debout recommends its being covered with a powder formed of equal parts of quinine and charcoal; and when the tumour is large, and there is much putrefaction, he adds an equal part of chloride of lime. In all cases he finishes the dressing by covering the entire abdomen with a thick layer of carded cotton. All trace of the hernia does not disappear when the cure is complete, for the fibrous tissues forming the umbilical ring have been so distended that a prominence remains which must be supported by a light bandage.

Fig. 7.

Fig. 8.



In pediculated or irreducible hernia, in which the rudimental intestine has run through all the phases of its development in the

umbilical pouch, a cure by the unaided powers of nature is impossible, for however small the mass of intestine may be, it is irreducible. The phenomena of the spontaneous cure take place, but the retraction of the peritoneal covering only results in its rupture, and the opening of the sac, followed by peritonitis, from which the child dies. Dr. Debout knows of no instance in which a cure of such a case has been effected; but as death without surgical aid is inevitable, he does not hesitate to recommend the early adoption of an operation for the reduction of the hernia. Two methods of operating present themselves—one in which the intestine is not only returned, but the edges of the opening are pared and brought together by suture; and another, in which the intestine is merely returned, and the parts allowed to undergo the process of spontaneous cure, as in cases of reducible hernia. "The chances of death being," he says, "in proportion to the extent of the wound, the practitioner ought to content himself with incising the abdominal wall, in the direction of the linea alba, and, the reduction of the hernia being effected, reuniting the wound with points of suture. The superior parts of the coverings may now be excised, and their base drawn together by a ligature; and if this do not suffice to maintain the edges of the opening in contact, recourse may be had to the use of adhesive plasters, as in the cases of Hey, Buchlotz, Hamilton, &c." In such cases we should not, he says, despair, the peritoneum is not more susceptible of inflammation in a child than in an adult, and the chances of success will be in proportion to the promptitude of the surgical intervention.

Dr. Debout concludes his most valuable monograph by detailing a number of cases, from his own practice, and various other sources, illustrative of the principles he has laid down. We regret that we cannot extract these in full, we can only make the following analysis of them, for the benefit of those of our readers who may not be induced to refer to the paper itself:—In 17 cases the hernia was reducible, and in eleven of these it was entrusted to the unaided powers of nature. In two the children died from the effects of local peritonitis, and in the others the cure was perfect, many of them having been seen in perfect health at the end of periods varying from 3 to 18 years. From one of these, case five, under the care of Professor Stolz, the illustrations here numbered 2, 7, and 8, showing the form of the tumour and the resulting cicatrix, have been taken.

In three cases recorded by Storch, Hey, and Buchlotz, the edges

of the opening were drawn together by adhesive plaster, after the hernia had been reduced, and cures ensued. In one where the tumour measured seven inches ("*mesure du Rhin*") in diameter, Bael, after having reduced the mass, surrounded the tumour with a ligature, and, after a prolonged suppuration, the child recovered. In another, Hamilton, after having reduced the hernia, applied a ligature on the base, and maintained the edges of the opening in contact by means of two silver pins and adhesive plasters, and the cure was complete in a few days.

In one case, related by Cruveilhier, the edges of the opening were pared, and united by the aid of points of suture, and a cure effected; but, Dr. Debout thinks, "in spite of the operation, rather than as its consequence."

A case of irreducible hernia, in which M. Giraldès operated in the manner recommended by Dr. Debout, is also related; but the parents would not permit the operation to be performed till after peritonitis had set in, and the child died on the fourth day.

We have to thank Dr. Debout for the opportunity of illustrating this review by electrotypes from his original blocks, and we cannot conclude without again expressing our sense of the great value of his highly practical and most important paper. By applying himself to the true source of all progress in medicine and surgery, the careful observation of the natural history of disease, and the means adopted by the system for its removal, Dr. Debout has placed on a scientific basis the treatment of congenital umbilical hernia.

The Diseases of the Prostate, their Pathology and Treatment, comprising the Second Edition of "The Enlarged Prostate," and a Dissertation "On the Healthy and Morbid Anatomy of the Prostate Gland," to which the Jacksonian Prize, for the year 1860, *was awarded by the Royal College of Surgeons of England.* By HENRY THOMPSON, F.R.C.S. London: John Churchill. 1861. 8vo, pp. 364.

A WORK, one portion of which has been so stamped with the approbation of the profession as to require a second edition, and the other portion of which is what our continental brethren would term an "*ouvrage couronné*," requires no great amount of praise at

our hands to recommend its perusal to the surgeon anxious to acquire information on the special topics of which it treats: in fact, in our opinion, the title page of the book carries with it its own review; and, were it not that we consider a few extracts from it may afford both pleasure and information to our readers, we should have been tempted to announce the appearance of the work, and the honourable circumstances under which its debut has been made, satisfied that the result, as a matter of course, will prove equally satisfactory to both author and publisher. As it is, we have culled a few passages from our author which will, we trust, give our readers a sample of his quality, and, at the same time, but whet their appetites for further information, and induce them to make themselves masters of a book from the perusal of which we ourselves have derived both pleasure and instruction.

Mr. Thompson's work is divided into two parts; the first of which is devoted to the description of the anatomy of the prostate, taking into consideration its topographical and structural anatomy, as also stating a most extensive series of facts in connexion with its weight, size, and morbid conditions. The second part follows as a sequel to the first, and treats of the diseases to which the prostate is subject; including, principally, acute and chronic inflammations and abscesses, hypertrophy of the prostate; with an exhaustive discussion of its anatomical characters, causes, symptoms, diagnosis, treatment, and effects, as regards the functions of micturition, prostatic concretions, and calculi; the relation between hypertrophied prostate and calculus, &c., &c.

If there be one idea more cherished than another in connexion with the anatomy of the prostate, if the existence of one division of the prostate be accepted as a more orthodox article of belief in the student's anatomical catechism, and indeed in the practitioner's also, than another, it is the middle lobe of the prostate gland. Since the days of Sir Everard Home its position has been laid down for us on our anatomical charts; it is the rock on which our efforts to relieve the distended bladder in retention of urine, dependent on enlarged prostate, have been so frequently endangered or wrecked; in life and in death its existence has been equally admitted; in disease, and in pathological specimens has its presence been insisted on; and yet, what does Mr. Thompson tell us :—

"II. The 'Third' or 'Middle' Lobe.—Lying between the posterior borders of the lateral lobes is a stratum of tissue uniting them throughout

their length. As it approaches the base, this stratum becomes thicker, and sometimes has a rounded form, as if it were a distinct isolated and independent formation, perforated about the middle by the ejaculatory ducts. It is that moiety of the stratum which lies nearer the apex to which has been applied the term 'isthmus,' or 'posterior commissure,' while the thicker part, situated at the base, has received the appellation of 'third or middle lobe,' and its existence as an independent lobe is regarded by most authorities in this country as a fact in normal anatomy. This, however, I am compelled to call in question, and shall give the reasons for doing so in detail. The idea appears to have originated with Sir Everard Home, who accorded to the part in question the title of 'third' or 'middle lobe,' after five examinations of the organ by dissection, performed by Sir Benjamin—then Mr.—Brodie, and he announced the result as the discovery of 'a middle lobe,' to the Royal Society, Feb. 20, 1806; the inquiry having been first instituted, to use the words of his paper, only two months before. 'Previous to this investigation,' says Sir E. Home, 'it was not known to me that any distinct portion of the prostate gland was situated between the vasa deferentia and the bladder. These ducts were considered to pass in the sulcus between the two posterior portions, in close contact with the body of the gland.' Notwithstanding this remark, the vasa deferentia had been described as perforating the posterior portion of the prostate, by several of the anatomical authorities of the time, as E. Home afterwards learned, a fact admitted in his first volume on 'Diseases of the Prostate,' published five years subsequeutly, viz., in 1811. Respecting the five dissections spoken of, the author says, 'the appearance was not exactly the same in any two of them,' The first preparation appears to have been from the person of an elderly patient, 'who had died in consequence of this part *being diseased:* the nipple-like process was very prominent.' In the next, 'there was *no apparent glandular substance*' at all in the spot indicated. All that is said of the third is, that 'there was a lobe blended laterally with the sides of the prostate gland;' but that there was really no distinct portion marked off as a lobe is clear, from the importance attached to such a formation being detected in the two subsequent cases. The most distinct appearance was found in two subjects of 24 and 25 years of age respectively; and upon the condition of the organs in these (and not in five) his account seems to have been based, and from these two cases the existence of a law was thus hastily deduced. Whatever may have been their condition (and it must be admitted that, at all events, no change similar to that of senile enlargement was likely to have been present in either), the existence of a distinctly marked portion of a spheroidal form, situate between the ejaculatory ducts and the verumontanum, at the inner meatus of the urethra, is so rare an occurrence as to mark an unusual state, whether congenital or acquired. Had the researches extended to a large number of bodies, it is certain that

these cases must have appeared to be exceptional, as my own inquiries have proved such to be. Any appearance of a lobe in this situation must be regarded as belonging, not to Normal, but to Morbid Anatomy, a slight development of it being usually attended with some signs of obstruction to the function of micturition. And so far from being overlooked, it is in this manner that it has been regarded, after very careful examination, by the earliest labourers in Pathological Anatomy who have left to us the records of their observations. Thus Morgagni, in that vast collection of cases which forms his work, 'De Sedibus et causis Morborum,' refers to it in several places as to a morbid growth causing retention of urine; in one of which he describes it, from his own dissection, in the following remarkably precise terms:—'A roundish protuberance, of the bigness of a small grape, covered over with the internal coat of the bladder. What this protuberance was I readily supposed: and by forcing the knife into it, I cut through this and the contiguous prostate gland at the same time, length-ways, and showed that it was of the same nature with the gland; that it was very evidently continuous from it, and that there was no doubt but, if it had grown out to a greater degree, it must have been a very considerable impediment to the discharge of urine.' In another case he describes the same appearance, pronouncing it 'beyond a doubt an excrescence of the prostate gland.' He quotes several similar cases from Valsalva, Thomas Bartholin of Padua, and Valisneri, who indeed speaks of an enlargement, 'as it were, a kind of lobe, from the glandular substance' (of the prostate) 'which rose up within the bladder, of the shape and size of a walnut, and not on the anterior part, but on that which lies adjacent to the intestinum rectum.' He enumerates others, mostly from the 'Sepulchretum' of Bonetus, besides alluding prospectively to another case which subsequently appeared in the succeeding letter of his own work; and then makes the following generalizations from the whole.

"'If you attentively examine those examples which I have pointed out,' . . . 'you will observe that they were all from old men; and in like manner, if you examine all my observations, in which there was the beginning of an excrescence, you will find that this was found to grow out in the very middle of the internal and upper circumference of the gland, posteriorly; but whether all these things happened by chance, or otherwise, future observations will show.'

"Some considerable time after this, that future was realized; Morgagni returned to the investigation of this subject, and the cause of his doing so is of great interest in relation to the present inquiry. It appears that 'a celebrated anatomist' of the time (Morgagni refers, without doubt, to Lieutaud, although he omits all mention of his name, as he states to be his invariable custom when he proves a contemporary to be in error) declared that an eminence at the neck of the bladder was not a morbid growth, but a small part quite natural, and common to all bodies, calling

it the 'uvula vesicæ.' Morgagni therefore devotes a great part of the sixty-sixth and the whole of the seventieth letters to the refutation of this view. In the former, stating that, during forty-four years as anatomical professor, he had most carefully dissected, at Padua, sixty or seventy bodies, and found it only in four; that he had made vivisection of a dog for the express purpose of seeking it there, but in vain, and that he had decided that it could be nothing but 'a morbid excrescence of the prostate gland appearing in old men,' . . . 'not rare, but not so frequent.' That Valsalva, Pohlius, and his friend Santorini, regarded it in the same light, the latter presenting it in a drawing as a body 'prominent in diseased bladders,' besides referring to it as 'a circumstance which is diseased and unfrequent, and does not deserve to be exhibited as perpetual and constant, to the great detriment and misleading of younger practitioners.' The seventieth letter Morgagni devotes to the purpose of giving the result of his forty-fifth year of teaching anatomy, in relation to this very subject, stating that he had dissected, in public, five subjects, and in none of them, although he had sought most carefully, was there any trace of this 'roundish protuberance,' or 'uvula.'

"John Hunter similarly regarded it, after independent examinations of the organ, stating that 'a small portion of it (the prostate) which lies behind the very beginning of the urethra, swells forward like a point, as it were, into the bladder, acting like a valve to the mouth of the urethra.'

"It seems remarkable, considering the very slight grounds upon which the existence of a distinct third lobe as a normal and ordinary constituent of the prostate, was affirmed by Sir Everard Home, that it should have been so generally received without question by English anatomists to the present day. Its existence is denied by most French observers. Cruveilhier expresses the general opinion when he says that the ejaculatory ducts, being received into a groove or channel in the substance of the prostate, a portion of variable size is indicated by them, but that it has no title to be called a lobe. It is not, he says, an isolated piece, and should be called 'the median portion.'

"The following is the result I have obtained after carefully prosecuting numerous dissections of this part.

"First.—I cannot find in healthy bodies, below 50 years of age, any formation in the situation described, capable of being recognised as a distinct 'third' or 'middle' lobe, and am compelled to conclude that any marked prominence there, which appears to possess independent characters (as regards size or form), must be considered abnormal or morbid.

"Secondly.—There is unquestionably a thick uniting stratum of tissue between the lateral lobes, which is sometimes slightly thicker in the middle line, at the vesical or basic end, than at its borders, where it becomes blended with those lobes. It is at this thickest part, three or

four lines below its urethral aspect, that there exists a perforation for the entry of the ejaculatory ducts. But in many cases this thickening in the median line does not exist; and it is to be remembered that the part in question immediately underlies the uvula, which is not a portion of the prostate, but a prominence caused by crossing interlacement of muscular fibres from the inner coat of the bladder, and sometimes prone to be unduly developed. If this fact is not borne in mind, a confounding of the two structures may arise, and the error of regarding them as one will lead the observer to attribute, inaccurately, a middle lobe to the prostate; and I think it has not unfrequently happened that the existence of a distinct middle lobe has been affirmed after simple inspection of the interior of the bladder, without further examination, which alone can determine, in all cases, whether a small projection at the neck is due to hypertrophy of the tissues of the uvula, or of the prostate, or to a small tumour embedded in the organ at this point.

"But, thirdly.—The posterior commissural part in question does possess a specific character, which distinguishes it from other parts—a character which appears to have been less prominently regarded, perhaps, than it has deserved to be, in the discussion of its title to the name of third lobe, and which appears to entitle it to some distinguishing appellation. Moreover, this character seems to be connected with that tendency to enlarge, which this part of the organ undoubtedly possesses, and which will be fully discussed hereafter. It is this, that the portion in question certainly contains a larger proportion of glandular structure than most parts of the entire organ. Thin slices from this portion, placed under the microscope and compared with slices from other parts, demonstrate this very clearly.

"Considering, then, all the terms which have been proposed to designate the part in question, there does not appear to be sufficient ground for continuing the term 'lobe.' On the contrary, it appears desirable to adopt that which the French anatomists have employed, viz., 'the median portion,' since it is sufficiently accurate; and it is perhaps not wise, without ample justification, to alter a term already used in that modern language, which, in scientific literature, is perhaps, at the present day, the most widely understood. If, retaining the terms 'lateral lobes' as universally accepted, we apply to the part anterior to the urethra (standing position of the body) the term 'anterior commissure'—a part which is about one inch in length; and the term 'posterior commissure' to that which corresponds to it for the same length behind the urethra, it leaves that thicker portion of the organ which lies behind the verumontanum, which is penetrated by the ejaculatory ducts, which is largely perforated by glandular structures, and which is prone to great increase of size in age, to be designated by the term, 'median portion,' conformably to the practice already pointed out. This part, it may be added, it

has been proposed by Mercier of Paris to call the 'portion susmontanale,' as indicating its position behind the verumontanum. The above reasons for adopting the simple and hitherto better-known term of 'median portion,' apply, perhaps, equally to this proposal also, although there is no other objection to its adoption by any who prefer it, as it indicates pretty clearly the part intended by the writer."

We have to apologize to our readers for the introduction of so long an extract, but when an error is asserted as existing in our received anatomical views, we are bound to give the grounds on which the assertion is based; to such of our readers as are familiar with the opinions of continental anatomists this view of the anatomy of the prostate will not seem as *heretical* as otherwise it may appear.

Passing from the anatomy of the prostate, we shall select, for our readers' perusal, some of our author's observations on the diseases to which it is subject—and first, of hypertrophy. In enlargement of the prostate, this so-called middle lobe is generally indicated by surgeons and pathologists as the principal seat of disease, as a result of most careful investigation of the subject (with the details of which, however, we shall not load our pages, though given most fully in the work under review). Mr. Thompson gives us the following summary:—

"The general inferences which must be drawn from all the facts adduced are—

"(α) That the lateral lobes and the median portion, or middle lobe, are equally liable, or nearly so, to be affected with hypertrophy.

"(β) That the posterior commissure is generally involved with the preceding enlargements, and in proportion to the extent which they manifest.

"(γ) That the anterior commissure is not frequently affected, but nevertheless is so in rare instances.

"(δ) Lastly, that development takes place at about an equal rate in each of the three principal divisions; in some cases the lateral lobes appearing to enlarge more rapidly than the median portion; in others the contrary effect is found, and perhaps in a rather larger number of cases than in the preceding."

As to the causes that predisposes to enlargement of the prostate, Mr. Thompson gives us a candid admission, to which we also are coerced to subscribe; and we agree with him, that such a confession of ignorance is far more likely to advance the cause of scientific

surgery than conjectures, however plausible, that after all have no substantial foundation. He writes:—

"It appears to me, however, that in order to accept the etiological views of this affection which are at present current, a good deal must be taken for granted; and that if we require a fair amount of evidence before we admit into the category of causes the circumstances and conditions usually recognised, we shall probably reject them all, or very nearly so. However discouraging to the practitioner such a result may at first sight appear—however unsatisfactory such a confession of ignorance may be deemed by the student—we may rely upon it, that, if it be a true one, it is the necessary and important preliminary step to a better state of knowledge on this subject. The admission of a single circumstance into the list of causes which cannot be sustained there by something better than a fanciful belief, conventional custom, or by, it may be, the 'impression' or the 'conviction' of an author, unsupported by testimony, must assuredly become a stumbling-block in our progress towards truth. Better were it to sacrifice the apparent completeness which often seems to be thought essential to a pathological treatise, if it can only be obtained by collecting all the suggestions and speculations that have ever been associated with the subject in the literature of the past; and rather to exercise—however much the literary character of the work may appear to suffer—a vigilance, lest we admit too much, than an anxiety to press into our service every line resembling a contribution, under the semblance of information on the subject. Nothing would be easier, on such a principle, than to swell this chapter into a volume, and for the simple reason that so little is known of its subject, so much conjectured."

Notwithstanding this open confession, Mr. Thompson submits, for our perusal and consideration, the opinions propounded on this point by our ablest writers, such as Hunter, Wilson, Bell, Cooper, Brodie, Coulson, Desault, Mercier, Civiale, &c., &c.

That portion of his work, however, which is "therapeutical" in its character, doubtless will command the largest share of the practising surgeon's attention. Theoretical and anatomical questions, of necessity, will attract the consideration of those specially engaged in such pursuits, but the hard-working practitioner too seldom has time to devote to the solution of such knotty points; not so with anything that bears on a remedial nature. These must command his serious attention, and ably will he find them handled in Mr. Thompson's masterly treatise. Although we fear that we have exceeded our usual limits for quotation, we shall submit, for our readers' perusal, this one passage, in which a remedy, easily

procurable, is mentioned, the use of which may appear novel to many:—

"An infusion of the underground stem of the Triticum Repens (dog's grass, or couch grass) is an agent from which I have derived much advantage. For 12 months I have used it largely, both in private and in hospital practice, in numerous cases of constant and severe irritation of the bladder from any cause; and in a certain proportion it has been of great value. It has long been prized in these circumstances in some country districts; and it is officinal as a diuretic, and as a 'diet drink' among the French. My first knowledge of it was derived from a country patient suffering from severe stricture, and the frequent and painful micturition of which he was the subject was obviously more effectually subdued by this remedy, which he had long taken for the purpose, than by any medicinal means I could devise. Accordingly, I have tested its powers as above stated; and my belief is, that it is of more value in the cases described, and in those of renal calculus especially, than in prostatic irritation. Nevertheless, in these cases also, where the indications are to lessen the frequency and the pain of micturition, it is unquestionably useful; often affording relief when buchu, pareira, uva ursi, and other infusions, have failed. The mode of administering it, which I have adopted, is the following. One ounce of the dried underground stem, popularly called root, is infused in a pint of boiling water for an hour, the liquid is strained off and taken when cool; from half a pint to a pint to be taken in 24 hours. It may be used as a vehicle for many other agents, but I have almost invariably given it alone, with the view of ascertaining its effects. It is somewhat diuretic, and perhaps slightly aperient."

1.—*Come oggi le Affezioni Scrofolotubercolari siansi fatte piu comuni, Considerazioni Storiche e Mediche.* Di A. CORRADI, Prof. di Patologia nella R. Universitá di Modena.

On the Modern Increase of Scrofulo-Tubercular Disease, considered both Medically and Historically. By A. CORRADI, Prof. of Pathology in the Royal University of Modena. Pp. 75. Quarto. Bologna: Gamberini & Parmeggiani.

2.—*In che modo le Diatesi o Disposizioni Morbose nei Popoli si mutino, e come entrino nella formazione dei sistemi Medici Dissertazione.* Del Prof. ALFONSO CORRADI.

In what Manner Morbific Tendencies or Diathesis Change, and how they enter into the Formation of Medical Systems. A Dissertation of Prof. A. Corradi. Pp. 43. Quarto. Bologna: *ut supra.*

Professor Corradi is already favourably known in this country through the pages of our Journal. Two years ago we reviewed an excellent memoir of his, on the decrease of gout in modern times. We then had much pleasure in making his acquaintance, and we are very glad to have the opportunity of renewing it now. We would, however, before commencing our analysis of the two memoirs, the titles of which are placed at the head of this review, express, as warmly as possible, our utter condemnation of all pamphlets in *quarto*. It is bad enough to have to deal with a volume of such a surface when it is a couple of inches thick, but a *quarto pamphlet* is worse and worse—it is a perfect typographical nuisance. Whilst, two years ago, Prof. Corradi was affording us abundant historical evidence of the diminution of gout in modern times, as compared to its prevalence in the middle ages, he now endeavours to prove that scrofulo-tubercular diseases have, on the other hand, increased; and as, in the first instance, he concluded that the decrease of the gouty diathesis was mainly due to the spreading of civilization, and consequent increase of agriculture, by which a cereal form of diet superseded the more highly azotized food which previously had been supplied by the countless herds of deer and swine that, in the middle ages, swarmed through the trackless forests then covering the greater part of Europe, and of which we still have interesting specimens in some parts of Bohemia and Transylvania; so, likewise, he believes that now a preponderance of a vegetable food over an animal, as is the case everywhere for the popular masses in civilized Europe, is the cause of the present increase of scrofulo-tubercular affections. If we were in possession of carefully constructed tables giving us the numbers of the population of even one country in Europe—France, for instance—together with registries of the annual deaths, and their causes, for the last 500 years, it would be comparatively an easy matter to ascertain whether tuberculosis has diminished or increased within that cycle of time. But such do not exist; the most reliable, and at the same time the longest, series of records of deaths, and their causes, are contained, we believe, in our own registries (we mean English); but we doubt that they extend over much more than 250 years. Prof. Corradi has, however, amply availed himself

of their valuable aid, so far as they could shed any light upon the subject; but, of the times antecedent to them, he has derived his information mainly from the remarks of the medical writers of the middle ages. At the present time, throughout civilized Europe, more than one-fourth of the deaths is attributable to tuberculosis in some shape; and in England alone, the Professor makes the calculation that one individual dies every 10 minutes of some one form of that disease. He calculates that out of 100,000 inhabitants there die annually, of phthisis alone, 323 in London; 382 in Paris; 296 in Berlin; 291 in Turin; 357 in Philadelphia; and, at page 12, he quotes from Heberden, that during the last century the deaths from phthisis in London regularly increased from 300, at the commencement of the century, to 400 towards the middle, up to 500 at the end of it, out of the same general mortality. This opinion he further corroborates by some statistics obtained from Woolcombe, who wrote in 1808. More ancient information upon this subject he derives from earlier writers, beginning with Rangone, of Ravenna, Prof. in the University of Padua, and who, writing in about 1558, makes no mention of phthisis among the numerous diseases to which the Venetians were subject. Baccio, physician to Pope Sixtus V., writing of the diseases prevalent in his day, mentions scabies, impetigo, lepra, and podagra, but not one word about anything resembling phthisis. But as we approach our own times, tuberculosis comes upon the scene, and in such a manner as to make it evident that it was not an accustomed enemy. The edicts of Cardinal Carafa, 1783, tell us that, 80 years before, phthisis *was extremely rare in the provinces of Ferrara;* but we infer that it had very much increased by that time, and that the idea that it was *contagious* was firmly established in those days, for we read in one edict the following words:—"Even the remote risk of the death of one man deserves the most jealous and anxious cares of those who govern;" and here follow directions for the purification and disinfection of the houses and furniture of those who died of phthisis. Having thus satisfied himself that phthisis has decidedly increased in modern days, the learned Professor passes on to consider the several causes which may have brought about such an increase. He honourably acquits vaccine from having had any participation in it, and he cannot think that climate has much to say to it, as he finds that it prevails in most countries. "Phthisis reigns equally at Naples, Vienna, Paris, London and St. Petersburgh; while, in the country parts of those same nations, it is

much less common." He also exonerates syphilis from having in any way originated tuberculosis, in opposition to Alibert, Petit, and others who held that opinion. "Were our ancestors so pure a race?" says the Professor. "I shall not speak of the Greeks and Romans, who offered up incense to prostitution, but I shall look to more modern times; was not depravity at its height in the sixteenth century, and when did prostitution carry its head higher? Even the sacred places were invaded. 'In hac etiam urbe (Roma), meretrices ut matronæ, incedunt per urbem seu mula vehuntur: quas assectantur de media die nobiles familiares Cardinalium clericique.' Giacomo Vitriaco, writing about Paris, in 1560, says: 'Simplicem fornicationem nullum peccatum (the Parisians) reputabant. Meretrices publicæ ubique per vicos et plateas civitatis passim ad lupanaria sua clericos transeuntes quasi per violentiam pertrahebant. Quod si forte ingredi recusarent, confestim eos sodomitas post ipsos conclamantes dicebant. Illud enim fœdum et abominabile vitium adeo civitatem, quasi lepra incurabilis et venenum insanabile, occupaverat, quod honorificum reputabant, si quis publice teneret unam vel plures concubinas. In una autem et eadem domo scholæ erant superius prostibula inferius.' Syphilis was well known in those days; many Physicians reaped abundant harvests by treating it. Steitz wrote a treatise on it, and dedicated it to a lady abbess; while a Dignitary of the Church, Bishop Torella, published in Rome a treatise upon the same. 'Tract. cum consiliis contra Pudendagram seu morbum gallicum. Romæ, 9to, 1497.'"

Syphilis, then, did not originate tuberculosis; neither did vitiated and impure air, though Baudeloque considered it one of the principal sources of this disease. What, then, is the cause? —the change of diet. "We require by nature azotized aliments," . . . "and we find that where tuberculosis is rare, animal food is abundant." From considering the invasion of tuberculosis as due to the change of diet, we naturally pass to the analysis of the second memoir on our list, upon the change of diathesis, and their influence on the treatment of disease. Prof. Corradi does not attribute those changes to atmospheric influence, but refers them to the same causes which have supplanted gout, and introduced scrophulosis. Our change of diet from a highly azotized food to a food containing little or no nitrogen, such as the potato, Indian corn, or rice, has produced, more or less, a leuco-phlegmatic tendency, to which is to be attributed, according to him, the non-

toleration of venesection in these times; and he facetiously remarks that if at present a new monastic order were to be formed, it would be superfluous to enter in its regulations any directions for the periodical bleeding of the monks, as used to be the case in many orders, as, for instance, the White Monks of Padua, who had to be bled regularly five times a year to keep their passions in subjection.

Both memoirs are full of interest and erudition, and show a vast amount of reading and research.

1.—*The Health of the Royal Navy, considered in a Letter addressed to the Right Hon. Sir John Pakington, G.C.B., M.P.* By GAVIN MILROY, M.D., F.R.C.P., &c. London: Robert Hardwicke. Pamphlet, pp. 69.

2.—*Public Health in Relation to Air and Water.* By W. T. GAIRDNER, M.D., F.R.C.P., Edinburgh, &c., &c. Edinburgh: Edmonston and Douglas. Fcap. 8vo, pp. 360.

WITHIN the last 20 years the consideration of the best means of preserving the public health has necessarily become a part, though it is to be regretted a small part only, of the education of medical men. The laws which regulate health have been studied carefully, and the results of that study, and of experience, have not only been laid before the members of the profession, but have also been impressed upon the public mind, so that sanitary matters may now be said to have been reduced almost to a science, and are generally understood by the educated portion of society. It is a cause of regret, however, that this vital subject, though its great truths are almost universally received, has not yet commanded that attention, especially from the authorities and from ruling bodies and corporations, which its importance demands.

It is true that in some places, and under certain coercing circumstances, the heads of departments and bodies corporate have listened to the advice, in some cases, the remonstrances, of medical men, have stepped aside from the hackneyed usages or abuses of routine, and have adopted the salutary principles of preventive medicine; and, in every instance where they have so done, the benefits that have accrued to the locality, and to those who were exposed to the noxious influences, have been apparent and decided, so manifest

and unquestionable indeed, that the most stolid and indifferent had to admit the utility of the measures. Now, with such examples before them, and these have been very numerous and duly recorded, one would imagine that men in authority would have been only too glad of opportunities to adopt known means to preserve health and life; we fear, however, that an examination of the vital statistics of cities and towns throughout the United Kingdom, and of the public services, would show a strange and culpable neglect of sanitary laws. This is the more extraordinary when we remember that it is not the expenditure of life alone which is in question, but what, alas! seems of more value in the eyes of the worldly man—the lavish waste of money; for it has been demonstrated, over and over again, by sanitary reformers—that in every life sacrificed, through neglect of the laws of public health, there is, independent of the value of life to the individual, which no one can reckon, a direct and estimable loss to the community of a certain amount of its wealth. There is, in fact, for every life so wasted an immediate monetary loss in some way or other, and, in a large proportion of cases, an accumulative expenditure also. Hence it is the more surprising that the subject of public health has not obtained more attention from those who have the means and the power of testing it fully, and of putting its ascertained principles in active operation. However, like every other question of the kind which is based upon truth, though its principles may be slowly acknowledged and obeyed, its doctrines will finally prevail, and men in authority will, as a matter of course, and for their own protection, if not from the higher promptings of humanity, regard sanitary science as of the first importance, and assign it a prominent place among their official regulations: then may man reasonably expect to live to old age, and the public services—the army and navy—shall no longer be sacrificed on the altar of official stupidity and neglect—shall no more be decimated, as they now are, by preventible disease.

The preceding observations are introductory to a brief notice of the two publications, the titles of which stand at the head of this paper. We have carefully looked into both of them, and are decidedly of opinion that they are calculated to effect much good. The letter of Dr. Milroy, a gentleman well known to the profession and the public as an able sanitary reformer, is worthy of all praise, directed as it is to the improvement of a branch of the public service which claims our liveliest sympathies; and being addressed to a distinguished statesman, who, when at the head of the

Admiralty, showed not only an intimate knowledge of the requirements of our naval officers and seamen, but also, by his active interference, effected many important improvements, it is likely to stir up the existing authorities and heads of departments in the navy to pursue the wise course adopted by Sir John Pakington when first Lord of the Admiralty.

Dr. Gavin Milroy, in his able and philanthropic letter, concisely and clearly points out the prevailing causes that materially affect the health and lives of our seamen in the royal service, and, by consequence, in the mercantile marine also, and he likewise shows the steps that should be taken to avert the existing evils! A perusal of his interesting letter exhibits at once the existence of these evils, their magnitude and fatal consequences, while it likewise points out the remedies. In fact, his letter is a fine practical lecture to the officers of the naval service, but more especially to the executive. The medical officers, we presume, do not require much instruction in the matter, nor do they require to be urged to carry out any measures likely to conduce to the improvement of the health of ships' crews; but they *do* require the countenance of the superior officers, the admirals and captains, and the hearty co-operation of the other representatives of the executive corps; and all this they would readily obtain if the Admiralty were to listen to proper advice, and then, in accordance with that, were to issue and enforce the due observance of comprehensive sanitary regulations. To the Admiralty, then, the country and the naval service must look for action in this most important matter; that Board which has usually had the character of being obstructive in its operations whenever medical questions have arisen, must turn over a new leaf, and learn salutary lessons from the statistics of the health of the navy which are annually laid before them. But if, in the arrogance of office and the stupidity of routine, they will not listen to these quiet monitors they must be made to hear the voice of the nation, they must be brought to their senses through the medium of the Legislature, and we know no better text for an aspiring member of the House of Commons than the sanitary evils that exist in ships of war, and the lukewarm conduct of the Board of Admiralty. But we trust that the Admiralty will, of their own accord, direct special attention to the subject of Dr. Milroy's letter, and see that every effort shall be used to protect the health and lives of the hardy and gallant seamen who man our royal fleets and mercantile navy. We trust that each member of the Board has a copy of this letter, and that

its perusal will stir up in his bosom the same feelings of sorrow that it has awakened in our breast, and a like warm sympathy for the noble fellows who are embarked in the service of their country.

The work of Dr. Gairdner is, as might be expected, replete with information, and well calculated to afford the public salutary advice. It consists of seven comprehensive lectures, with copious notes, which illustrate, in a highly popular style, several of the most important points of sanitary science.

This course of lectures was delivered by Dr. Gairdner to his medical class; and in his introductory discourse he states that he had a "two-fold object" in view. "*First*, and chiefly, to convey some of the elementary principles of modern sanitary science to those to whom I have taught the art of healing; to complete, as I think it ought to be completed, the cycle of sciences, on which that art of healing rests.

"*Secondly*, to establish, if I can, a cordial understanding between the medical profession and the public in this matter of public health; to claim for the noble, open-minded, and generous profession, to which I am proud to belong, the rightful influence that springs from its natural position in relation to sanitary matters; an influence which I am sure will always be accorded to it, whenever the true conditions of public health are rightly understood." To advance in some degree both of these objects, without sacrificing the one to the other; or rather, let me say, to advance them by calling each of them in its turn to aid the other, is what I have set forth, to my own mind, as the thing to be done."

A careful perusal of Dr. Gairdner's able lectures will convince the reader that the learned author has fulfilled his task in a most worthy manner; he has thrown together, in a lucid style, some of the most important facts in regard to public health; and so clear and convincing are his illustrations and arguments, that we are satisfied that the lecturer on sanitary science could not find a better text-book for his guidance. It is much to be regretted that teachers of the healing art, especially those who lecture on the practice of medicine, do not devote more time than they usually do to the inculcation of hygienic principles upon the minds of their students; for although, as we stated at the outset of this notice, medical education in the present day embraces a knowledge of preventive medicine, still there is much of a most important character, in connexion with vital statistics, that is left entirely unnoticed. Dr. Gairdner, therefore, as a teacher of medicine, has

not only set an excellent example to his fellow professors, but has also furnished a very complete guide to those of them who will follow in his footsteps. But his lectures are calculated to do much more, they are well adapted to create inquiry in the public mind, and to give a fresh impulse to the study of a subject that has probably fallen not a little into neglect, now that cholera and epidemic fever have not, for a lengthened period, made their dread visitation amongst us.

We trust, however, that Dr. Gairdner's book will have the effect of once more awakening public attention, and directing it to a subject that should never be put in abeyance; for if there be any purely secular question that demands our inquiry before all others it is surely that which refers to life, and the enjoyment of life and health, the preservation of both being consequent, in a great measure, on the removal of preventible disease.

A Handbook of Volumetrical Analysis. By Robert H. Scott, M.A., T.C.D. London: Longmans. 1862. Cr. 8vo, pp. 107.

It would be impossible to over-estimate the importance of this department of chemical science, which has grown up almost altogether within the last ten years. That it is held in high estimation by the most distinguished chemists of the day is sufficiently proved by the fact that they have dedicated their time and talents to its development. Of these, we may particularly mention Bunsen, Liebig, Mohr, Pelouze, and Margueritte. By this method of analysis we are enabled, in a few minutes, to obtain results which would require as many hours according to the ordinary method by weight. Notwithstanding the rapidity with which these analyses can be executed, they yield nothing in point of accuracy to those performed by the aid of the balance. The determinations of iron by permanganate of potash, of iodine by arsenious acid, of bleaching powder by arsenious acid and iodine, and of silver by chlorine, leave nothing to be desired on the score of accuracy. Mr. Scott has, therefore, conferred an inestimable boon on the chemists of this country by presenting them with the present admirable work, which is the only monograph on the subject in the English language.

Mr. Scott has exhibited in this work very considerable talent for scientific composition. The style is, throughout, remarkably clear, and the arrangement simple and scientific. The student will find his comprehension of the subject greatly facilitated by numerous well-executed woodcuts. The value of this work is, in our opinion, greatly enhanced by the fact that its pages are not encumbered with descriptions of untrustworthy analyses, but are confined altogether to those of undoubted merit.

Mr. Scott divides all volumetrical analyses into three subdivisions, according to the chemical effect upon which they are based:—

1st. Saturation analyses.
2nd. Oxidation and reduction analyses.
3rd. Precipitation analyses.

Under the first division come the determinations of acids and alkalies. These are based on the chemical fact that an equivalent of any acid exactly neutralizes or saturates an equivalent of any alkali; and conversely, that an equivalent of any alkali exactly saturates an equivalent of any acid. Thus, we know, before having recourse to experiment, that 63 grains, or one equivalent of oxalic acid (63 being the equivalent number of this acid considered as monobasic $C_2 O_3$, 3HO) will exactly neutralize 40 grains, or one equivalent of hydrate of soda (NaO, HO), and 56 grains, or one equivalent of hydrate of potash (KO, HO), &c. To enable us to estimate the value of alkalies, we have, therefore, only to prepare a dilute acid of known strength, which is usually called a test-liquor. The acids commonly used are sulphuric and oxalic; of these the latter is, in our opinion, more convenient. Mr. Scott, however, gives the preference to the former. The preparation of a test-liquor of this acid is exceedingly easy. We have only to dissolve a known weight of it in a known volume of water. The acid must, however, be rendered perfectly pure by repeated crystallizations. In order to determine the value of an alkali, say a solution of soda, by means of this test-liquor, we introduce it into an accurately graduated tube, or burette, as it is usually called, and add it, drop by drop, to the weighed soda solution, previously coloured blue by a small quantity of an infusion of litmus. The moment the point of saturation is reached, a single drop of acid from the burette will change the colour of the solution to red. This indicates the termination of the process. The volume of our acid test-liquor, which has been required to produce this change, can now be seen from the

burette; and, as we know the quantity of acid it contains, we can readily determine, by calculation, the amount of soda this quantity indicates. If, for example, 63 grains of oxalic acid have been employed, then our alkaline solution contains 40 grains of soda. If the quantity of acid used has been smaller, a simple rule of three sum will enable us to ascertain the corresponding amount of soda. Potash and ammonia may be determined in the same manner; and, by a very slight modification of the process, the alkaline carbonates and bicarbonates.

The capabilities of our test-liquor are by no means yet exhausted. By its aid we are enabled also to estimate the value of acids. With this view we prepare a solution of pure soda, and determine the quantity of hydrate of soda in a measured volume of it, in the manner just described. The strength of the soda-solution having been thus ascertained, nothing is easier than the determination of acids by its aid. The acid to be analyzed, say sulphuric acid, is weighed, diluted, and coloured red by the addition of a few drops of an infusion of litmus. The soda test-liquor is then gradually added to the solution, till its colour changes to blue. This indicates that the point of saturation has been reached. The quantity of soda required to produce this change gives, by calculation, the amount of sulphuric acid in the specimen, 40 grains of soda (NaO, HO) being equivalent to 49 grains of sulphuric acid (HO, SO_3). Muriatic, nitric, and acetic acids may be determined in a similar manner

The second subdivision of volumetrical processes comprehends all analyses in which oxidation or reduction takes place, or both together. The agents employed for oxidation are permanganate and bichromate of potash, iodine, &c. Those for reduction are protoxide of iron, arsenious and sulphurous acids, hyposulphite of soda, zinc, &c. The determination of iron is, perhaps, the most important in this division. According to this method we estimate the quantity of iron present in any solution, by the amount of oxygen necessary to raise it from the state of a proto to that of a per-salt. The body usually employed to furnish the oxygen is permanganate of potash, or chameleon mineral as it is commonly called, the solution of which has a deep purple colour. One equivalent of this (KO, Mn_2O_7), by giving up five equivalents of oxygen, peroxidizes 10 equivalents of protoxide of iron:—$10(FeO)+5O=5(Fe_2O_3)$. It also converts the same number of equivalents of protochloride of iron (containing excess of hydrochloric acid) into perchloride; the necessary chlorine in this case being obtained from the excess of hydrochloric acid

present, which is decomposed by the oxygen of the "chameleon" thus:—$10(FeCl)+5O+5HCl=5(Fe_2Cl_3)+5HO$.[a]

In order to determine an unknown quantity of iron by means of this body, it is necessary to prepare a solution of it, and ascertain, by experiment, what volume of the solution is required to convert a known quantity of pure iron from the state of protochloride to that of perchloride. This is readily done by dissolving about 10 grains of pianoforte wire (which is sufficiently pure) in excess of hydrochloric acid, with as little exposure to air as possible. The protochloride thus formed is diluted with water, and the chameleon added, drop by drop, from the burette, the solution being agitated all the time. The peroxidation of all the iron, and consequently the termination of the process, are indicated by the solution assuming a permanent wine-red tint, which is produced by the presence of a trace of undecomposed chameleon. This colour could not appear as long as the solution contained the smallest quantity of protochloride, as this would, by decomposing the "chameleon," destroy its colour. We have now only to ascertain how many cubic centimetres of our solution have been employed. If 30CC, then 30CC=10 grains of iron.

Having determined the strength of our test-liquor we can readily estimate, by its aid, the value of hematite and other iron ores. A weighed quantity of the hematite is dissolved in hydrochloric acid, by which means we obtain a solution of perchloride of iron, $Fe_2O_3+3HCl=Fe_2Cl_3+3HO$. This we must, of course, reduce to the state of protochloride before we can employ our oxidizing agent upon it. The reduction is effected by heating the acid solution with pure zinc, till a drop ceases to colour sulphcyanide of potassium, a proof that all the iron is reduced to the state of protochloride. The solution is now diluted and the chameleon added as before. If 30CC of the chameleon have been employed, then our specimen contained 10 grains of iron. The results are very exact. This process can be applied to the analysis of highly complex solutions, containing, besides iron, silica, alumina, lime, &c. The chameleon searches out the iron, oxidizes it, but neither affects nor is affected by the other ingredients.

By means of the above process we are enabled, indirectly, to

[a] The complete decomposition which takes place in haloid and oxygen acid salts is expressed by the following equations:—$10(FeO, SO_3)+8(SO_3)+KO, Mn_2O_7=5(Fe_2O_3, 3SO_3)+KO, SO_3+2(Mno\ So_3)$. $10(FeCl)+8(HCl)+KO, Mn_2O_7=5(Fe_2Cl_3)+KCl+2(MnCl)+8HO$.

estimate a multitude of very important bodies. A preliminary process is, however, necessary in these indirect analyses, in order to prepare for the application of the chameleon. This varies according as the substance to be determined has oxidizing or reducing properties. The preparation used with an oxidizing body may be illustrated by the case of peroxide of manganese. This method is based on two chemical facts:—1st. That one equivalent of peroxide of manganese, when undergoing solution in hydrochloric acid, evolves one equivalent of chlorine, $MnO_2+2HCl=MnCl+2HO+Cl$; and 2nd, that one equivalent of chlorine converts two equivalents of protochloride of iron into perchloride, $2(FeCl)+Cl=Fe_2Cl_3$. One equivalent of the peroxide, then, in dissolving, evolves enough chlorine to peroxidize two equivalents of iron. Therefore 56 grains, or two equivalents of iron, raised from proto to perchloride, indicate 43·57, or one equivalent of peroxide of manganese. The process is thus conducted:—About 15 grains of pianoforte wire are dissolved in hydrochloric acid, without exposure to air. A weighed quantity of the peroxide (about five or six grains) is introduced into a flask, strong hydrochloric acid is added, and a bent tube attached by means of a perforated cork. The open end of the tube is then plunged into the iron solution, and heat applied to the flask containing the manganese. The chlorine evolved changes a portion of the protochloride of iron into perchloride. The amount of iron unaltered is determined by the chameleon. This, deducted from the whole amount employed, gives the quantity of iron converted into perchloride by the manganese; 56 grains of iron = 43·57 grains of peroxide of manganese. Somewhat in this way we may analyze nitric, chloric, and chromic acids. Mr. Scott employs, in these determinations, sulphate of iron and ammonia, which is, in many respects, preferable to the metallic iron.

The preparation required for the "chameleon" in the case of reducing bodies may be illustrated by the determination of zinc ores. This method is founded upon the fact that freshly precipitated sulphuret of zinc reduces perchloride of iron to protochloride in an acid solution: $Fe_2Cl_3+ZnS=2(FeCl)+ZnCl+S$. Therefore 56 grains, or two equivalents of iron, reduced to the state of protochloride, indicate 32·5 grains or one equivalent of zinc. The analysis is thus conducted:—The zinc ore is dissolved and converted into sulphuret of zinc. This is washed, and introduced into a flask with an excess of perchloride of iron. The flask is then corked, and digested for some time at a gentle heat. As soon as the action is finished, the

contents of the flask are largely diluted, and the amount of iron reduced to the state of protochloride determined by the chameleon. Copper, sulphuretted hydrogen, &c., may be analyzed in this manner.

After the chameleon, perhaps the most important oxidizing agent employed in volumetrical analysis is iodine. By it we can readily determine arsenious and sulphurous acid, hyposulphite of soda, &c. These analyses are based upon the same principle as those performed by the chameleon. We estimate the quantity of arsenic in any liqour (to take a particular case) by ascertaining the quantity of oxygen necessary to raise it from a lower to a higher state of oxidation, from arsenious to arsenic acid. The iodine acts indirectly as an oxidizing agent in presence of arsenious acid, by decomposing water, the oxygen of which it liberates by combining with the hydrogen, according to the following equation:—$As\,O_3 + 2HO + 2I = As\,O_5 + 2(HI)$. 254 grains, or two equivalents of iodine, therefore, liberate a sufficient quantity of oxygen to convert 99 grains, or one equivalent of arsenious, into arsenic acid. We can ascertain, in a very striking and accurate manner, when this change is complete, by mixing a small quantity of a solution of starch with the arsenious acid before adding the iodine test-liquor. As long as the iodine is converted into hydriodic acid, according to the above equation, the solution remains colourless; but the moment all the arsenious is changed into arsenic acid, this conversion ceases, and the free iodine combining with the starch communicates a blue colour to the solution.

To prepare a test-liquor of definite strength we have only to dissolve a known weight of pure iodine in a known volume of a solution of iodide of potassium. In order to analyze arsenious acid with this we must dissolve a weighed quantity of it in a solution of carbonate of soda, introduce some starch-paste, and add the iodine solution gradually from the burette till the liquor assumes a permanent blue tint. We then calculate the amount of iodine in the volume of the test-liquor employed, and from this the amount of pure arsenious acid in our specimen. 254 grains of iodine indicate 99 grains of arsenious acid. The sharpness of the results obtained by this method are highly gratifying.

Of all the reducing agents employed in analysis by measure, the principal of which we have already enumerated, arsenious acid is the most valuable. By its aid we are enabled to determine chloric and chromic acids, chlorine, chloride of lime, bromine, iodine, &c.

It is, however, usual to employ at the same time our oxidizing test-liquor of iodine. The principle of this method is the converse of that just explained. We determine the amount of free chlorine in any solution (to take a particular case) by ascertaining the quantity of arsenious acid necessary to convert it into hydrochloric acid by the decomposition of water, according to the following equation:—As $O_3+2HO+2Cl=As\ O_5+2(HCl)$; 99 grains, or one equivalent of arsenious acid is, therefore, sufficient to convert 71 grains, or two equivalents of chlorine into hydrochloric acid. In order to prepare a test-liquor of definite strength, we dissolve a weighed quantity of pure arsenious acid (if it sublimes without leaving any residue it is probably pure) in a known volume of a solution of carbonate of soda. To analyze a solution of chlorine with this we introduce it into a burette, and add it gradually to the chlorine, till a glass rod dipped into it ceases to impart a blue colour to iodized paper, made by impregnating paper with a solution of iodide of potassium and starch-paste. As long as the liquor contains a trace of free chlorine it will communicate a blue colour to the paper, the chlorine combining with the potassium and liberating the iodine. We now learn from the burette the quantity of arsenious acid which has produced this change, and calculate the amount of chlorine from it; 99 grains of arsenious acid indicating 71 grains of chlorine. In these analyses it is usual to employ, also, the iodine test-liquor, as already stated. If we wish to use it in this case we add a measured excess of the arsenic solution to the chlorine water, that is, more than necessary to convert all the chlorine into hydrochloric acid, and afterwards estimate the amount of this excess, by mixing the liquid with starch-paste and adding the iodine test-liquor till a blue colour is produced. In calculating the results we, of course, deduct the amount of arsenic in excess from the whole amount used. The results are very accurate. In analyses by this method the two test-liquors should be comparable with each other.

Mr. Scott's third division of volumetrical analyses contains processes of precipitation. The most important analyses under this head are those of chlorine by means of silver, and *vice versa*, of silver by means of chlorine. Nothing can be easier, or give more exact results, than the determination of chlorine (which must be in the form of soluble chloride) by means of nitrate of silver. The reaction which takes place with common salt, is expressed by the following equation :—$NaCl+AgO,\ No_5=AgCl+NaO,\ No_5$. 170 grains, or one equivalent of the nitrate, are therefore sufficient to pre-

cipitate 35·5 grains, or one equivalent of chlorine. The test-solution is prepared, by dissolving a known weight of pure silver in nitric acid, evaporating to dryness, and heating cautiously till fusion. The fused nitrate is then dissolved in a known volume of water, and introduced into the burette. The chloride to be analyzed, say common salt, is weighed, dissolved in water, and mixed with a drop of a solution of chromate of potash. On adding the test-liquor, the silver combines with the chlorine, and forms a white precipitate of chloride of silver. The moment, however, the last trace of chlorine is removed from the liquor, and not before, the precipitate changes to red, from the formation of chromate of silver. This indicates the termination of the process. One equivalent, or 170 grains of the silver-salt, are equal to 35·5 grains of chlorine.

The determination of silver by means of common salt, gives also very accurate results. The test-liquor is made by dissolving a known weight of pure common salt, in a measured volume of water. This we add gradually from the burette to a weighed quantity of the silver to be analyzed, dissolved in nitric acid. The silver-solution should be rather acid, and should be well shaken after each addition of the common salt. The operator should also wait till the chloride of silver has completely subsided before adding a fresh quantity of the test-liquor. As soon as a drop of the test-solution ceases to render the liquor turbid, the process is finished. The burette is now read off, and the amount of silver calculated, 58·5, grains of common salt indicate 108 grains of metallic silver. All silver determinations are made in this way by Pelouze, in the Hotel de la Monnaie, at Paris.

Medical men will be glad to see in Mr. Scott's book, a description of Liebig's method for the determination of urea, and of Barreswell's for the determination of grape sugar.

In the foregoing sketch, we have endeavoured to give our readers an abstract of the subject which Mr. Scott has so ably handled in his present work.

Asclepiad—Clinical Essays. By B. W. Richardson, M.D. Churchill, London. 8vo, pp. 272. 1862.

From the preface of this work it would appear that it is the intention of the author "to bring out in occasional volumes, after

the manner of the volume now presented, a series of essays on subjects relating to medical science in its various departments." He does not bind himself to any regularity of publication, but hopes "that the volumes will, on an average, be issued half-yearly, or at least annually."

The book before us is the first of the promised series, and contains seven essays on the following subjects:—

1. On Subclavian Murmur.
2. On a Diseased Condition of the Nails.
3. On Reduplication of the Second Sound of the Heart.
4. Contributions towards a more perfect Clinical History of Scarlet Fever.
5. On Pulsatile Pulmonic Crepitation.
6. On Uremic Coma.
7. On Cardiac Apnea.

The first essay treats of subclavian murmur and its value as a diagnostic of disease. Of its importance the author says—"I cannot better prove it than by stating the fact, that, within the last three years, four cases have been referred to me as cases of aneurism in the thorax, which have proved, on further examination, to be nothing more than well-marked examples of subclavian murmur; cases, as we shall see hereafter, very innocent in their nature when compared with aneurismal disease." During a space of nearly two years the author sought diligently for the subclavian murmur in every case which presented itself at the Royal Infirmary for diseases of the chest; 2,000 patients of all ages were examined, of whom 1,011 were females, 989 males; the murmur was found well marked in 51 cases, and of these 48 were males and only 3 were females—the oldest being 74 and the youngest 18 years of age. Of these 51 cases, 18 were suffering from chronic bronchial affection, 12 were suffering from phthisis, 10 from anemia, 7 from dyspepsia, 3 from heart disease, and 1 from a combination of phthisis with bronchial disease. Dr. Richardson observed that the occupation of the patients had a considerable influence apparently in the development of this murmur. "In the vast majority of cases the patients were employed in manual labours, in the exercise of which the arms are thrown forwards and downwards. Thus there are no fewer than 15 cabinet-makers in the list, 4 carpenters, 2 sawyers, and so on." This subclavian murmur which, according to our author, is undoubtedly arterial, varies in different cases and at different times in the same case also. "It may be a soft musical

coo; . . . . it may be a loud, coarse, almost snorting gush, conveying an unpleasant thrill to the ear; . . . . it may be loud, yet so short, as to resemble a sound produced by a quick blow with dull resonance." The thrill, which is heard with the ear, is at times unmistakably felt with the finger, and, on one occasion, "the vibration could be seen when the finger was removed." The principal value of this murmur, as a diagnostic sign, is expressed at p. 15, where, having advanced the view, that it is caused by pressure, upwards and forwards, of the subclavian artery, "by a portion of solidified lung raised during inspiration," the author says:—

"In these examples there is, I believe, mostly, some solidification at the apex of the lung on the side on which the murmur is heard; the solidification may be due to deposit of tubercular matter, or to enlarged and indurated bronchial tubes. In such instances the character of the murmur varies according to the condition of the lung-substance. . . . Where tubercular deposit is laid down in one spot, immediately in contact with the artery, and where the deposit is hard, or where there is enlargement and induration of bronchial tubes immediately behind the artery, the murmur, which will still only be heard during inspiration, is sharp and shrill, or shrill and grating. It will continue so long as the breath is being held, and will disappear immediately on expiration. The murmur thus caused is often well marked in the early stages of phthisis, but disappears in later stages, when the deposit of tubercle has undergone the changes of softening and removal."

But this murmur was detected in persons in whom no pulmonary disease existed, and even in persons in perfect health. "In all these examples," says the author, "the pressure exerted on the artery is, in my opinion, brought about by the action of the subclavius muscle, and is the result of those occupations in which the arms are being constantly thrown forwards and downwards, as occurs in wood-planing, hand-sawing, French-polishing, and the like." This subclavian murmur may be confounded with aneurismal murmur, with valvular murmur, with pleuritic friction, and with bronchial coo. From all of these it may, however, be easily distinguished, and the rules for doing so are clearly laid down from p. 18 to p. 28.

What are the bearings of this diagnostic sign in practice? "Unless caused by mechanical occupations, it indicates the existence of some pressure exerted on the artery, by a structure posterior to the vessel

—in most instances we must infer by the lung. Thus it affords one diagnostic indication, both of bronchial disease and tubercular deposit . . . . but in phthisis the murmur may prove one of the earliest indications of tubercle, and in two cases where there were general symptoms of phthisis, and a phthisical history, but where the ordinary physical signs of the disease were utterly wanting, I was supported strongly in the suspicion of actual tubercular deposition by the prominence of a subclavian *bruit.* In both these cases the suspicion was confirmed by after events." We venture no opinion on the foregoing assertions, the reasoning is fair, we know Dr. B. W. Richardson to be an accurate and intelligent observer, and we trust that this diagnostic may be verified by others, as any symptom which could help us to ascertain the presence of tubercle in its earliest stage, would be a desirable addition to the means we already possess.

The peculiar diseased condition of the nails, which forms the subject of the second essay, though not common is yet sufficiently frequent. We have seen at least three examples of it. We do not think it of syphilitic origin, as in the three cases which were under our care for several months, we could not trace any history of syphilitic taint. We agree with the author in considering arsenic by itself, or in combination with iodine and mercury, as in Mr. Donovan's well known triple compound, the best remedy in such cases; it completely succeeded with one of the two in which we tried it; in the second case there was considerable improvement, when we lost sight of the individual; in the third case the patient was suffering from hypertrophy of the heart, and died shortly after our seeing him, so that no time was afforded for a trial of either remedies. The author adds nothing in this essay to our previous knowledge of the disease.

The third essay, which we now mean to consider, treats of "reduplication of the second sound of the heart;" a rare symptom of but little value in a diagnostic point of view, and which the author ascribes to a want of synchronism in the closing of the aortic and pulmonary semilunar valves. He quotes Drs. Cockle, T. C. Webb, and Walshe, as having carefully studied this phenomenon; but makes no allusion whatever to Professor Stokes, who long ago called the attention of the profession to this reduplication of the second sound, and was, we believe, the first to assign to it its proper value and diagnostic import.

In the fourth essay, which consists of contributions towards a more perfect clinical history of scarlet fever, the author tells us

nothing new. He commences by showing, with the aid of the mortality tables of the Registrar-General, that scarlatina before the age of two is not rare, as had been asserted by some authors; out of a total of 12,962 deaths from scarlet fever in children under five years of age—occurring in the county of Kent in 1843, in London in 1845, 1846, and 1848, and in England in 1847—there were 4,163 deaths of babes under two years. As to the prevailing idea that females are more liable to the disease than males, he denies its correctness, and finds, from his own practice, that both sexes are equally subject to it; but, on referring to the mortality tables, there is a slight excess of deaths among females. As to its recurrence in the same individual, we are all aware that it does sometimes happen; but, as a general rule, we believe, with Willan, that scarlatina once had gives immunity to the patient from further attacks, just as much as small-pox. Concerning the poison of scarlatina and its mode of propagation, our author expresses himself as follows:—"The poison of scarlet fever, specific in its nature, is the most subtle of all zymotic poisons. It travels on material objects with infinite ease, and may be laid up in material objects for any period; yet, left to itself, it seems incapable of transit to any considerable distance, the air above not wafting it more than a few feet, if so far;" and at page 93 he gives an interesting account of several clear cases of transported infection. The eminently contagious character of the poison of scarlatina is well known, and we remember an anecdote of the late celebrated Dr. Graves, who, when asked what he would recommend as the best method of disinfecting a house after this disease, answered, after pausing a little to consider, "burn the house down." In the treatment of this disease the author proposes nothing new, and confesses that a specific for its cure is yet to be discovered; he does not even allude to Belladonna, and seems to place more reliance on endeavouring to eliminate the poison by the kidneys and the skin, than by any other means. He also recommends the administration of ammonia or acetic acid, theoretically to keep the blood in a fluid state, so as to prevent death by the deposition of fibrin in the right cavities of the heart. "It is of importance," he adds, "to administer the agent in small and frequently repeated doses, so that the blood may always contain the remedy." If ammonia is selected for the treatment, he advises it to be administered until "it is very distinctly presented in the breath," and describes a simple method by which it can easily be detected. This sounds very well; and so far as acetic acid is

concerned, if used to acidulate water, and given as a drink, we have not the least objection to it; but, with regard to ammonia, we would advise great caution: it will aid to keep the blood fluid, no doubt, but it will also imperil its vitality by destruction of the blood cells if pushed too far; and in a blood disease, like scarlatina, in which the safety of the patient so much depends on the successful elimination of a poison from the circulation, we would be extremely cautious before risking the further impoverishment of the blood by any substance that might endanger the vitality of its corpuscles.

The gargle he recommends in fetid ulceration of the throat is one of which we have no experience; but we shall try it on the first opportunity. It is composed of six ounces of solution of peroxide of hydrogen, containing 10 volumes of oxygen; tincture of myrrh one ounce; and rose water five ounces; to be used *ad libitum.*

The fifth essay treats of "Pulsatile Pulmonic Crepitation," by which the author means "a stethoscopic sign, in which, without any indication of pneumonia or tubercle, a crepitant sound, connected with pulsation, is presented to the ear." We cannot do better than let Dr. Richardson describe the new sound himself; we have never met with it, and, as he says at p. 130, "in the matter of treatment, a knowledge of the existence of pulsative crepitation is of no affirmative value, or at most of little," it is evident we have had no great loss; but as this abnormal sound appears to have been also noticed by Dr. Willis in *l'Union Médicale* of June 16th, 1860, we shall describe it in the author's own words:—

"About six weeks before the death of the patient (a lady suffering from emphysema of the left lung, with dilatation of the bronchial tubes, and mitral patency), after the subsidence of a very severe attack of dyspnea, during which I was unable to make a physical examination of the chest, owing to the oppression which the act produced, I was surprized, when the examination could again be borne, at hearing over the cardiac region an auscultatory sound, which was not only new to this special case, but new to my ear altogether. . . . . An inch below the nipple, and bearing to the left side, there was a superficial sound which could be localized by the mouth of the stethoscope, and which, when present, obscured the systolic rasp altogether at that single point of observation. The sound was so superficial that it seemed directly beneath the stethoscope. . . . . It was not a friction sound, it was not a murmur, it was not an ordinary crepitation, it was rather a coarse crackling noise, resembling that which is produced by the urning

of dried gorse, or the tearing of a piece of calico. The period of the sound was also peculiar, it was irregular as to time; and sometimes, with the stethoscope over the spot where it was most marked, it would be absent during an entire action of the heart. . . . . When the sound did appear, it was synchronous with the systolic rasp and clouded it. . . . This new sound was in some measure influenced by respiration. . . . . When the patient expired, and forcibly opposed the refilling of the chest with air, the new cardiac sound was absent altogether."

This lady died in a short time, and at the *post mortem* the author found "the pericardium adherent to the lung, and over the base of the left ventricle, to the left side, in the exact position over which the pulsatile crepitation had been heard during life, there extended a corner or tongue of lung. . . . . The appearance of the lung indicated that the structure had been subjected to compression at this overlapping point;" and the author concludes from this examination that the crackling was produced in the following manner: "the piece of lung thus placed was subjected to the impulse of the heart during its systole; and when the lung was inflated on inspiration the sudden compression produced by the impulse, elicited the crepitation by forcible expulsion of air from the air-vesicles." If this explanation be the correct one, and we consider it a very probable one, it is evident that this coarse crackling sound whenever it should happen to be heard, need not alarm the practitioner by its loudness as it would not appear to be otherwise formidable.

We have now reached the sixth essay—it treats of "uremic coma," and is the longest of the series, occupying no less than ninety pages. The author begins by saying,

"Uremia bearing in many instances a close analogy to other symptoms by known narcotic poisons, as well as to symptoms of apoplexy and even of epilepsy, the pure indications of uremic coma cannot be too early learned by the student, nor too carefully studied and recast by the matured practitioner. In the present essay, it is my object to speak only of the uremic condition as it absolutely presents itself in its most marked representation. As the patient appears when, comatose and convulsed, he lies the prostrated victim of uremic toxemia, I write of him, of his symptoms, of his pathology, of the immediate cause of his malady, and of the treatment which promises most certain and effectual relief."

Uremic coma might no doubt be mistaken for coma, arising from

the action of other poisons introduced into the system either accidentally or purposely, and at page 135, the author relates an interesting case of uremic poisoning, in which a curious inquest was held, and an analysis of the contents of the alimentary canal, and part of the viscera, performed—but he tells us nothing that was not already known. At page 143, however, where the author speaks of the treatment of uremic coma, he does teach us something new, and something which theoretically sounds well, but which we fear would prove very unsatisfactory in practice; he advises us to bleed the patient *pleno rivo*, but let us hear him.

"I attended a boy . . . 7 years old, through a severe attack of scarlet fever in 1850. . . . I heard nothing more of this child until January 21st, 1851, when I was summoned in haste at 10 P.M. I learned that the child, two days before, had been exposed to cold and wet, and that this exposure had been followed by suppression of urine. A little of the urine last passed had been saved for use, and was found to be albuminous . . . when I arrived . . . the body was cold; the pulse small; the convulsions extreme; the consciousness quite lost. . . . The pupils were widely dilated and fixed; the breathing was catching and irregular. I at once tied up an arm, and opened a vein freely. . . . I took no less than 16 ounces of blood from this little patient without a sign of syncope, and I left him composed and conscious. . . . On the 1st of February . . . the urine was passed freely, and was clear of albumen. On the 24th of February the child . . . was seized, after a hearty meal, with a repetition of his former symptoms."

This time he was not bled, only leeched, and on the following morning he died. The author regrets much that he was "induced not again to draw blood from the arm." At the *post mortem* the kidneys were found *pale* and *large*. Does he really believe that by again copiously bleeding this child of *seven years of age*, he would have saved his life? At page 145 the author recites another case, in which a copious bleeding in uremic coma restored consciousness, "while the blood was flowing the patient, for the first time in 70 hours, became sensible . . . (remained conscious for four days). . . . On the evening of the fourth day, the patient relapsed into the comatose condition, and on the following morning died." During that interval of four days, the patient, who had suddenly become comatose, and had hopelessly remained so for 70 hours, was enabled to settle his affairs. This is often a matter of no little importance, and to obtain the opportunity of effecting such arrange-

ment, the bleeding is very admissible. Ligature of the aorta, though in itself a fatal proceeding, has been deemed advisable under such circumstances, in order to prolong life even for a few hours; but bleeding, with a view to curing the patient, seems to us utterly fanciful, in the ordinary cases of uremia, though we could imagine a case of very acute renal dropsy, with suppression of urine, occurring in a plethoric subject, in whom bleeding from the arm might temporarily prolong life, and give time for other remedies to come into action, and eventually save it altogether.

We have now reached the seventh and last essay of the Asclepiad, which is to tell us about "Cardiac Apnea;" the author's definition is the following:—

"By the term cardiac apnea, I mean a form of breathlessness often terminating in death, which is due to the fact, that in the cases in which it is presented, the pulmonic current of blood is being withdrawn from the air. In this apnea, the entrance of air into the lungs may be entirely unobstructed . . . it has been noticed at different times by various names, such as 'angina pectoris.'"

Cardiac apnea is an old acquaintance with a new name, and as we do not admit as proven all the causes of it which are laid down by the author, neither do we approve of this new name, and still prefer the old "angina pectoris," which conveys at once the idea of the ailment, without involving a theory which yet remains to be established.

The favourite theory of the author is that "the fatal paroxysm is due to an arrest of the flow of blood through the *right* side of the heart," caused by "slight filamentous deposits of fibrin around the tendinous cords of the tricuspid or mitral valves." He brings forward as proofs of the correctness of his views several *post-mortem* examinations of subjects which had died of cardiac apnea, and in whom fibrinous deposits were found occupying the right auriculo-ventricular opening, and hence passing from the right ventricle into the pulmonary artery; we have no doubt that he often did, and we would always expect to meet with such in every case where there had been a *protracted death struggle*, such as generally occurs in death from angina pectoris; but we are not prepared to admit that these fibrinous deposits arise except as a result of the prolonged death agony. It appears also to us that some of the cases enumerated can scarcely be classed as cases of angina pectoris, the one at

page 235, for instance, which looks more like a case of spasm of the glottis, although quoted as an "illustration of cardiac apnea."

The chapter on the diagnosis of this disease, which the author characterizes as "an apnea with open air passages; not panting breathlessness, but suppressed breathing," is very good, and the descriptions contained in it are graphic. The chapter on treatment is equally good; the advice to apply warmth to the precordial region as quickly as possible, during a paroxysm, by carrying hot fluids into the stomach, can not be too much impressed. *In extremis* the author recommends "artificial respiration; . . . . the air should be gently introduced into the lungs, and, whenever practicable, should be raised in temperature to a degree equal, at least, to that of the body itself, 98° Fahr."

We have, at last, turned over the last page of the *Asclepiad;* with some of the articles we have been well pleased, with others, we have been, perhaps, disappointed; though, perhaps, more in consequence of our too great expectations from the pen of the able author of the essay on *The Coagulation of the Blood.*

We hope, notwithstanding, to have the pleasure of meeting with Dr. Richardson again; for we like original thinkers, even if we do not always subscribe to their views, or follow them in their deductions.

A System of Surgery—Pathological, Diagnostic, Therapeutic and Operative. By SAMUEL D. GROSS, M.D., Professor of Surgery in the Jefferson Medical College of Philadelphia, &c. Illustrated by 1227 Engravings. 2nd Edition, enlarged and revised. Philadelphia: Blanchard and Lea. 2 vols., 8vo, pp. 1062 and 1134.

THREE years ago (November, 1859), we noticed the first edition of this work in terms of high commendation; and the fact that we have now to announce a second edition, affords most satisfactory evidence of the correctness of the opinion we then pronounced. This new edition has been enlarged and revised; the number of illustrations increased from 936 to 1,227; and altogether no pains

have been spared to make it worthy of a continuance of the patronage it has hitherto received.

It cannot be expected that, in a work of such dimensions, and embracing every subject having any bearing on surgery, there should not be many points on which opinions may differ, and sections into which errors may have crept. Of the latter we must say we have met with very few. Of the former, we would instance the directions as to the treatment of simple fractures of the skull with depression to some extent, but not sufficient to be productive of symptoms of compression. In these cases, Dr. Gross says: "If the bone be forced down considerably, so as to impinge very decidedly on the brain; or if it be comminuted or jagged at the edges, the sooner it be raised or removed the better, since, if it be allowed to remain, it cannot fail to become a source of trouble either by exciting inflammation or by causing unpleasant secondary effects." The teachings on this subject of all British surgeons of the present day, we need scarcely remind our readers, are at variance with those of our American confrere; they being unanimous in forbidding any operative interference in cases of simple fracture where there are not decided symptoms of compression; and even in compound depressed fractures without confirmed symptoms of compression, it is by many considered better, as a general rule, to leave the bone alone than to attempt to raise or remove it. In the paper published by Dr. Murney, of the Belfast Hospital, in a late vol. (vol. xxxiii., p. 284), it is stated that of 124 cases of simple fracture with depression, in which the bone was elevated, 50 per cent. died; whereas, of 25 cases in which no operation was performed, only 28 per cent. proved fatal, though in many of the latter who recovered, symptoms more or less grave were present for a time—a statement that we would commend to Dr. Gross's consideration when preparing the next edition of his work.

Dr. Gross's acquaintance with surgical literature is most extensive and profound; and he has availed himself, very freely indeed, not only of the writings of others, but of their illustrations too. In one case, however, that has attracted our attention—dislocation of the first phalanx of the thumb on the dorsum of the metacarpal bone—he has endorsed the claims of countrymen of his own, Professor Crosbie, of New Hampshire, and Dr. Cutter, of Massachusetts, to the discovery of a mode of treatment originally described by Sir Charles Bell, and constantly practised in the hospitals of this city, where it is facetiously called the "homeopathic plan,"—a not

inappropriate name given to it, we believe, by Mr. Adams, of the Richmond Hospital. The procedure consists in first forcibly pushing the phalanx backwards till it stands at right angles with the back of the metacarpal bone, and at the same time pressing its base forwards till its articular surface is brought over the edge of that of the other, when sudden flexion towards the palm will accomplish the rest; and we are so convinced of its efficacy, that we are unwilling to allow the credit of its suggestion to be carried off from our countryman.

We cannot close this brief notice of Dr. Gross's most valuable and excellent compendium of surgery without again drawing attention to it, as we did in our notice of his first edition, as an evidence of the progress our American brethren are making towards establishing a literature of their own, and expressing our heartfelt wishes that the present deplorable internecine warfare may soon terminate, and leave them again free to pursue the arts of peace and humanity.

Further observations on the Structure and Treatment of Uterine Polypi. By ROBERT LEE, M.D., F.R.S., Obstet. Phys. to St. Georges' Hospital. Medico-Chirurgical Transactions, Vol. XLIV., for 1861.

WE have been induced by the very great importance of this paper, and the great value of the materials it affords, to make it the subject of a special notice, instead of embodying it in our review of the volume in which it is to be found. It contains the histories of 105 cases of uterine polypi, being all the cases of this disease that Dr. Lee has met with, both fortunate and unfortunate, and the histories of these are so recorded, as to enable each reader to study and draw conclusions from them, almost as if he had himself seen the cases: and we mean, in the remarks we have to offer, to lay before our readers the conclusions we have drawn from them, as well as those of the author.

The paper opens with a *résumé* of the author's previous observations on the structure of polypi; recapitulating the varieties he has described, which we may name the fibrous, the mucous, the cystic, the glandular, and the erectile. Dr. Lee further refers to his

paper in the Transactions for 1850, in which he says, that he demonstrated the manner in which the circulation of the blood is carried on throughout the substance of fibrous tumours of the uterus, and demonstrated that, like the walls of the uterus, they are everywhere pervaded by arteries and veins, and we presume he means that we should take the same description as being true of fibrous polypi. The remainder of the paper is taken up with the history of the cases, with a view to the recommending of the use of the ligature for the removal of polypi, and to the refutation of Dr. West's statement, as to the very high mortality arising from its use in Dr. Lee's own cases, as reported in his *Clinical Reports of Ovarian and Uterine Diseases*, published in 1853.

Of the cases now recorded many were not under Dr. Lee's own care, but were seen by him at some of their stages. There is a good deal of obscurity in the manner in which Dr. Lee sums them up. He treats them as a total of 103; in three, he says, no operation was performed, and the result is not known; in two the polypi disappeared spontaneously. Of the 98 cases which remained 5 died before any operation was attempted. After deducting these 10 cases there remain 93, of which 8 died and 85 recovered. Of the fatal cases one, No. 7, was in a moribund state when the ligature was applied; in another the polypus was in a sloughing state when the ligature was applied; in No. 11 the polypus was torn by hooks in an unsuccessful attempt to pull it down before the ligature was applied, and the patient died of peritonitis; and in No. 16 the patient died of peritonitis, before the ligature had cut through the pedicle. The following extract gives, in Dr. Lee's own words, an account of the treatment he adopts:—

"By an examination of this condensed history of 103 cases of uterine polypi it will be seen that, of the last fifty cases which have come under my own care, and in which the operation for the removal of the polypi has been performed by me, not one patient has died. The various methods of treatment, having been minutely described in the histories of the cases, do not require to be again detailed.

"Being fully aware that the greater number of large uterine polypi are fibrous tumours covered with the lining membrane and a portion of the muscular coat, and that these polypi have large arteries and veins distributed throughout their substance, in operating upon them I have not ventured to drag them out of the vagina with forceps of any kind, and divide their roots with the knife. The ligature has usually been applied in such cases with the bent rod, and the sloughing polypus has been

removed when the ligature was long in dividing the root. By this means all the evil consequences which could be produced by the vagina being long filled with a putrid mass have been completely avoided; and to this course I do think may be attributed, in a great degree, the uniform success of operations performed by me of late. Even when the polypi have been of comparatively small size, and the ligature has been applied with the double canula, I have not considered it safe to leave the ligature many days around the root of the polypus, when the circulation through the substance had been destroyed, and the mass of the tumour was in a sloughing condition. By twisting the canula firmly round, the ligature has frequently passed through the root, and the polypus has been removed several days before this would have happened if the ligature had been merely tightened. Even in cases where the root of the polypus has not yielded to this treatment, the ligature has been removed, and the dead polypus has come away harmlessly after some days, tepid water having been freely injected into the vagina. In all cases it has appeared of the utmost consequence to watch the condition of the uterine organs and of the general system, as it has been clearly proved that inflammation is the most common cause of death after the application of a ligature around the peduncle of a polypus."

On analyzing the cases recorded, more fully than the author has done, they seem to us not to bear out the remarks with which he has prefaced them, and neither to justify the very uncourteous remarks he has made about Dr. West, nor to prove the great superiority of the ligature over all other methods of removing polypi. His cases may be divided into eight classes, as follows:—

1. Removed by ligature	43	cases,	6	died.
2. „ „ and immediate excision	7	„	1	„
3. „ „ and subsequent excision	13	„	2	„
4. „ torsion	21	„	0	„
5. „ excision	3	„	0	„
6. Mode of removal not stated	2	„	0	„
7. No operation performed	12	„	5	„
8. Particulars not known	4	„	–	„
	105	„	14	„

Of the six patients who died after removal of the polypus by ligature, one was dying at the time of the operation, viz., No. 7 on register, two died from peritonitis, Nos. 11 and 16, and three from pyemia, viz., Nos. 40, 44, and 88. The patient who died after the

removal of the polypus by ligature and immediate excision, No. 20 on register, had the tumour in a sloughing state, and the passages inflamed before the ligature was applied, and symptoms of pyemia began with a rigor the day after the operation was performed; and two others, in whom a ligature was applied, and the polypus subsequently removed when it was in a sloughy state, viz. Nos. 34 and 37 died from pyemia. It is further to be observed, that of the cases treated with the ligature alone, and who recovered, one suffered from phlebitis, No. 28; and one from a pelvic abscess, No. 58; and one of the cases treated by ligature, and in which the polypus was afterwards twisted off, also suffered from phlebitis, No. 98.

Dr. Lee makes the assertion, that "of the last fifty cases which have come under my own care, and in which the operation for the removal of the polypi was performed by me, not one patient has died." The statement appears to be made somewhat loosely, at least it is not borne out by the register. On examination it will be found that of the last 51 operations recorded, but one patient died, and she was not operated on by Dr. Lee, but neither were eight others of the 51 cases; and if we take the last 50 cases recorded as having been operated on by Dr. Lee himself, we must begin with No. 33 on the register, and then it will appear that four of the cases died, viz., Nos. 34, 37, 40, and 44; but, as we have said, we believe Dr. Lee means us to understand that of the last 51 cases but one died, and he did not operate in that case.

One who criticizes the statements of others so freely as Dr. Lee does, and hesitates not to impute misrepresentation and concealment, should at least be careful as to the correctness of his statements as to his own success. We do not in the least suspect that there has been any intention to mislead in the present instance, but we cannot avoid expressing a hope that the error now pointed out will lead to a little more of true charity on future occasions.

The statement is one of great interest, and calculated to arrest the most profound attention; but the question arises, does this great success depend on the mode in which the operation was performed, or on the nature of the cases operated on? We have further analyzed the register to determine this question. We find altogether 89 operations recorded, out of which 9 deaths occurred, or nearly 10 per cent. We divide the cases into two series—the one consisting of the first 38 operations, and the other of the 51 last performed, and in the following table show how they were circum-

stanced as to the thickness of the pedicle and the mode of operation.

	First Series.		Second Series.	
Ligature:—				
Polypus small as a pea,	1 case	0 died	1 case	0 died
„ small, slender pedicle,	3 „	0 „	6 „	0 „
„ large, do. do.,	2 „	0 „	2 „	0 „
„ large, thick do.,	12 „	4 „	8 „	1 „
„ size of pedicle not stated,	1 „	1 „	6 „	0 „
Ligature and subsequent excision (after some days):—				
Polypus large, pedicle small,	2 „	1 „	0 „	0 „
„ do. do. thick,	1 „	1 „	8 „	0 „
„ small, do. do.,	1 „	0 „	0 „	0 „
„ size of pedicle not stated,	0 „	0 „	2 „	0 „
Ligature and immediate excision:—				
Polypus vascular or cystic, pedicle small,	2 „	0 „	0 „	0 „
„ large, thick pedicle,	2 „	1 „	1 „	0 „
„ size of pedicle not stated,	1 „	0 „	1 „	0 „
Excision:—				
Character of polypus not defined,	1 „	0 „	0 „	0 „
Lip of uterus?	1 „	0 „	0 „	0 „
Polypus long and pendulous,	0 „	0 „	1 „	0 „
Torsion:—				
Polypus small, hard,	0 „	0 „	3 „	0 „
„ do., soft,	5 „	0 „	10 „	0 „
„ large, do.,	0 „	0 „	1 „	0 „
„ double, small,	1 „	0 „	0 „	0 „
Torsion and immediate excision:—				
Polypus soft and vascular,	0 „	0 „	1 „	0 „
Operation not stated,	2 „	0 „	0 „	0 „
	38 „	8 „	51 „	1 „

It is at once seen, that of the nine deaths seven occurred in cases where the pedicle is stated to have been "thick." In one (No. 44 on Dr. Lee's register) the size of it is not stated, but as in it the ligature took some days in coming away, notwithstanding that the polypus was in a sloughing, disorganized state when it was applied; and in the other, No. 34, though the polypus was large the pedicle is said to have been "not very thick," and the ligature, had not cut through it on the sixth day, when it was twisted off, we may fairly infer that in all the fatal cases the pedicle was "thick," and that it is its thickness that determines the risk of the operation.

Now, it will be seen that the proportion of thick pedicles in the second series is less than in the first. If the 38 cases in the first series afforded 16 thick pedicles, the 51 of the second should have afforded 21·47; if, however, we assume that the pedicle was thick in all the cases where its size is not stated, the proportions will be equalized, we will have, in the first series, 18 thick pedicles, 26 in the second; but of the 18 patients in the first series, 7 died (excluding No. 34, "not very thick"), and of 26 in the second, only one died; we must, therefore, look for some additional circumstance to account for the fatality in the first series, and this we find in the mode of operation adopted, viz.:—that in the first series a putrefying mass was allowed to remain in the vagina, till it was thrown off by sloughing; and in the second the polypus was removed before putrefaction had made much progress. The fact that in each of the fatal cases except one, No. 7, where the patient was *moribund* when the operation was performed, the polypus is described as having become putrid, confirms this view.

In conclusion we must say, that Dr. Lee has not convinced us of the great advantages of the ligature over all other methods of removing uterine polypi, not even when combined with excision; but he has satisfied us, that the greatest danger of the operation arises from the putrefying of the polypus, and that any means that would remove the polypus at once, and at the same time prevent hemorrhage, such as the *écraseur*, which Dr. Lee has not used at all, is the best and most advisable.

Die Krankheiten des Ohres, ihre Erkenntniss und Behandlung. Von Dr. von Tröltsch, Praktischer Arzt und Privatdocent in Würzburg.

The Diseases of the Ear; their Diagnosis and Treatment. By Dr. von Tröltsch, Practitioner and Lecturer in Würzburg. Würzburg, 1862. 8vo, pp. 262.

In the number of this Journal for August, 1861, we gave a notice of a treatise on the anatomy of the ear, in its practical bearings, by Dr. von Tröltsch, the author of the work now before us, and alluded briefly to the apathy displayed by the profession at large upon the subject of aural surgery. We would again direct atten-

tion to this branch of medicine, by referring at length to the work whose title heads this article. The idea is still very general that aural diseases are not amenable to the ordinary rules of art, or the treatment of the qualified practitioner, and hence the quack and nostrum-monger are so frequently resorted to. The opinion of the public is, in some measure, attributable to members of the profession, some of whom have yet to learn that ear diseases are as curable as those of the eye or any other organ. We daily hear skilled and well-qualified practitioners mockingly inquire "what can be done for deafness?" and if we peruse the medical periodicals of the day, we not unfrequently find aural surgery treated of in a manner suitable only to the days before Saunders commenced his valuable researches in that speciality. We remember, not long ago, reading in one of the leading journals a report of several cases of deafness, in not one of which was there any mention made of the physical signs in the meatus or the membrana tympani, in not one of which the speculum had been employed. As well might we publish the report of diseases of the eye, of the chest, or of the uterus, without having recourse to those means which art has placed at our disposal for the elucidation of their morbid appearances. When we use those faculties of observation and reasoning in aural cases, which we do in other medical cases, we shall find that they are governed by the same general laws; that diseases of the ear are analogous to diseases in other parts of the body, and require analogous treatment. In this as well as in every other speciality, however, its practitioner must possess a well-educated mind, an observant eye, but, above all things, a thorough knowledge of general medicine, pathology and therapeutics; and without such knowledge, we believe no man has ever attained to real eminence in any speciality, or gained a lasting reputation. Every speciality possesses, no doubt, certain technicalities, which it is scarcely possible for the student to acquire during the brief time he spends in the wards of an hospital; but if the general knowledge of disease he gains be brought subsequently to bear on affections of special organs, his treatment thereof will be rational and not empirical.

Dr. von Tröltsch's work consists of a series of academical lectures delivered in Würzburg; a few of the subjects treated in which we now propose to consider.

The first which naturally is suggested is the means employed to see the external meatus and membrana tympani. Peter de la Cerlata, professor of surgery in Bologna, in the latter end of the

fifteenth century, was, we believe, the first to recommend a "speculum or other instrument" wherewith to inspect the meatus. The most useful instrument of this class is the tubular speculum, described by Mr. Wilde, in 1844, in the former series of this journal, and which von Tröltsch figures and recommends as the best of its kind. Its introduction is simple and painless, and with it we are enabled, by means of good sunlight, to inspect both the passage and the membrana tympani.

To meet the necessities of dark gloomy weather various lamps and artificial lights have been employed, none of which, however, answer fully the required purposes. Dr. von Tröltsch has devised a new method for illuminating the membrana tympani, which we have found very useful, and which we can recommend. It consists of reflecting, instead of transmitting directly, the ordinary daylight through the speculum, by means of a concave mirror about three inches in diameter, and of about five-inch focus, with a central aperture. This mirror is attached either to a handle like the ophthalmoscope mirror, or, in order that when necessary both hands may be free, to a spectacle frame like the laryngoscope mirror. The ear under examination must be turned away from the light. When we consider how many houses in cities are so situate that it is impossible to obtain a good direct sunlight through any of the windows, and the necessary position of the examiner's head, and also the impracticability of examining the ears of persons unable to move out of bed, we perceive the difficulties which the surgeon encounters in making an accurate diagnosis from the physical signs. In the great majority of cases the plan we have just mentioned meets all these difficulties. Although artificial light is, as a rule, inapplicable, there are occasions on which its employment becomes necessary—as, for instance, during the dense fogs which occur in cities, and at night—and then the mirror described above may be used with advantage to reflect such light. Von Tröltsch, who brought this method of illuminating the ear under the notice of the congress of German physicians in Paris, in 1855, says, that it was not until a subsequent period he became aware that a similar plan had been proposed, in 1841, by a Dr. Hoffmann, in Westphalia.

The chapters on *otitis externa*, and *otorrhea* are replete with sound practical information. The author follows in the footsteps of his illustrious British teachers in urging the necessity of checking and "drying up" discharges from the ear; he says he has "always found that in gradual cessation of the otorrhea the patient gained

in general health, whereas, on the contrary, when the discharge was allowed to continue undisturbed, many individuals sickened and finally died." If we bear in mind the anatomical relations of the parts, and remember that the bony partitions are very thin and delicate which separate the internal ear from the facial nerve, the transverse, superior petrosal and lateral sinuses, internal jugular vein, and internal carotid artery, and the middle lobe of the brain, and that these laminæ of bone afford but little resistance to inflammatory action, we can readily understand how a simple discharge, extending, either by continuity of surface or by metastasis, to the parts we have mentioned, may produce facial paralysis, phlebitis, inflammation of the dura mater and brain, and abscess in the latter. Hence, in every case of otorrhea we should be on our guard, for as long as the discharge continues the patient's life is scarcely insurable at the ordinary rate; as the author remarks, "every otorrhea may, under certain circumstances, become a life-endangering disease." The following case will illustrate these remarks:—We were lately called upon, between one and two o'clock in the morning, to visit a gentleman, about 27 years of age, whom we found suffering agonizing pain in the right ear, from which there had been occasionally a discharge for a long time previously. He had also suffered from a paroxysm of great pain during the previous night, and had, about 12 hours before, consulted Mr. Wilde, who made a careful examination of the ear, pronounced the case to be one of inflammation, and ordered the application of leeches round the meatus, which treatment had been carried into effect about three o'clock. There was no swelling, redness, or tenderness over the mastoid process; but, as the pain was intense, we applied several more leeches round the orifice of the meatus and also over the mastoid process, and ordered warm applications to the ear. About eleven o'clock we saw the patient, in consultation with Mr. Wilde, when we found him much relieved from the pain in the ear, but suffering from headache and nausea. There was no discharge from the ear at the time. He had, however, a remarkably quick pulse, and that peculiarly anxious "drawn" expression of countenance so well described in Mr. Wilde's *Aural Surgery*, as characteristic of certain serious inflammatory affections of the ear.

Under the circumstances it was thought advisable to place him, as soon as possible, under the influence of mercury, and small doses of calomel and blue pill, guarded with a little opium, were ordered to be administered every four hours. The serious nature of the

case was explained to the gentleman's relatives, as well as the circumstance of the necessity for the treatment adopted. The friends, however, perhaps considering our alarm groundless, informed us, at a later period of the day, that special advice was no longer required. Our fears, however, were not groundless, for the patient died within a week or ten days, we have been given to understand, with all the symptoms of auro-cerebral disease. It may have turned out one of those cases which have been so well described by Mr. Toynbee, and the operation for the relief of which, as shown by Mr. Wilde, was described in Sweden by the late learned Professor Retzius,[a] and in Germany by von Tröltsch.

Polypi, which constitute a frequent source of otorrhea, arise, according to the observations of Mr. Toynbee and Mr. Wilde, most frequently from the wall of the external meatus; von Tröltsch, however, states that these growths arise "most frequently from the mucous membrane lining the cavity of the tympanum and the upper part of the Eustachian tube." He has also seen them spring from the membrana tympani, and most frequently from the superior and posterior part of that membrane, close to its margin. "In one case," he says, "I found in the subject besides a polypus of the meatus, and another coming from the upper portion of the tube, a third, which, from its entire construction and its microscopical character, proved to be the membrana tympani in all its layers, degenerated into a polypus structure. I have frequently seen, in the living, excrescences which, from their form, position, and extraordinary sensibility, must be looked upon as abnormal growths of the entire membrana tympani." We have never yet met with such instances, nor do we believe they occur in this country. Small button-shaped excrescences are occasionally seen on the membrana tympani, which appear, at first sight, to have their origin in the membrane, but which prove, on more careful examination, to proceed from the cavity of the tympanum.

The greater part of the work under consideration is devoted to the *inflammations of the middle ear*, which are classified as acute and chronic simple, and acute and chronic purulent, "catarrh."

The author reminds us of the fact, that the mucous membrane lining the cavity of the tympanum and mastoid process, is inseparably connected to the periosteum, and acts both as mucous and periosteal membrane; and that hence every inflammation of that structure is, at the same time, practically also a periostitis. In

[a] Medical Press, Vol. xlii., p. 374.

consequence of acute or chronic simple inflammation of this membrane, the ossicula become frequently adherent, or connected by membranous bands, to one another, or to the adjacent parts, and the membrana tympani, in like manner, becomes connected to the ossicles or to the inner wall of the cavity; and in chronic inflammations, he says, "the mucous membrane becomes thickened, and the bones hypertrophied; whilst in acute inflammation it becomes ulcerated, and the bones atrophied and carious." Such inflammations frequently have their origin in the naso-pharyngeal space, and are propagated along the Eustachian tube to the middle ear, causing, in the tube, swelling of its membrane, secretion of mucous, and obstruction to the passage of air. The principal part of the treatment adopted and recommended by the author for these inflammations and their results, consists of the forcible injection of air through the Eustachian tube, the introduction of medicated vapours to the cavity of the tympanum, and especially the vapour of muriate of ammonia. With respect to the therapeutic action of the air-douche, he says:—

"It sets in motion, and drives into the mastoid process, or into the throat, every removable impediment, such as mucous or pus. It acts, therefore, as a purifier in bringing air to the tube and cavity; it removes the secretion, separates the walls of the tube which are, possibly, becoming affixed, and thus restores, momentarily, or for a longer period, the communication and balance of air between the pharynx and cavity. Furthermore, inasmuch as it makes tense and bulges out the membrana tympani, as we may preceive, any existing abnormal fixations or adhesions of that membrane are, necessarily, also made tense, stretched, and, under favourable circumstances, loosened or torn across."

A not unfrequent cause of sudden deafness is, no doubt, an accumulation of mucus, either in the Eustachian tube, or in the cavity of the tympanum, and which may be removed by Eustachian catheterism. From the displacement of such secretion into the mastoid process, and the filling up of its cells, we should have expected some unfavourable result, but the author states, that, in 25,000 cases, in which he performed catheterism, he never saw any such. Again, in acute simple catarrh of the middle ear, where there is agonizing pain, the membrana tympani red, and the parts internal to it, no doubt highly vascular, we should have hesitated to inject, forcibly, a current of air, did not the author assure us that it was rather beneficial than otherwise; and that the pain

became less, and the symptoms improved from the first application. We would compare the action of the air-douche, in inflammation of the membrane of the tube, and cavity of the tympanum, to that of belladonna or atropine in inflammation of the iris, inasmuch as they both, when employed in time, prevent adhesions of the adjoining parts. The author recommends it, not only as a therapeutic agent, but also as a means of diagnosis; he says:—

"Although we are able, in the great majority of cases, to diagnose 'chronic catarrh of the ear' from the appearance of the membrana tympani, yet catheterism and the air-douche are necessary, in order to make an accurate examination. By means of them we recognise affections of the tube—whether its mucous membrane is swollen and hypertrophied—whether it is normal in size or strictured—whether abnormal secretions are present in it, or in the cavity, &c. In many cases abnormal conditions of the membrana tympani are recognised only by means of the air-douche. By its employment we can judge of the elasticity of that membrane, of its mobility, or abnormal fixations, which we could not pronounce decidedly upon by any other means. But, granting the great importance of catheterism of the Eustachian tube, in the diagnosis of 'chronic catarrh,' we must guard against overestimating that operation, and drawing conclusions from its results, which, if critically analyzed, and from an anatomical point of view, are not warranted."

We would caution our readers against the indiscriminate use of catheterism of the Eustachian tube, for occasionally very untoward, even fatal results, have followed that operation. The cases in which von Tröltsch recommends its employment are those of acute and chronic otitis interna, without purulent secretion, and chronic otitis interna, with purulent secretion.

Otitis interna, with purulent secretion, has been found by von Tröltsch to exist in many infants which he dissected. These infants, of whom the youngest was 17 hours, the eldest one year old, resembled, and in many instances belonged to, that class of illegitimate, half-starved, neglected, foundlings, in whom are found atrophy, inflammation of the intestines, bronchitis, and venous congestion of the coverings of the brain and of that organ itself. In 48 temporal bones of such children he found the middle ear normal in 13 instances, the petrous portion carious in two, and the remaining 33 presented the anatomical features of purulent otitis of the middle ear and mastoid process. "The cavity of the tympanum, upper part of the tube, and the cells of the mastoid process, were filled with a greenish

yellow, creamy, or gelatinous-like fluid, which resembled pus in all respects, and presented, under the microscope, all its characters." These results are very remarkable; and although they are at present merely pathological facts, they will, no doubt, as science advances, become of practical importance The author suggests that a great mass of children, supposed to be suffering from meningitis, are, in reality, labouring under disease of the ear.

Fracture of the malleus is an accident of very rare occurrence. Von Tröltsch relates an instance which came under his own observation; another has been lately recorded by the distinguished and learned comparative anatomist, Hyrtl, to whom we are indebted for so many observations and researches in the organ of hearing. Mr. Toynbee possesses, in his great and valuable museum illustrative of the pathology of the ear, a specimen of fracture of the inferior extremities of the malleus.

Von Tröltsch enjoys the reputation of being at present the first aurist in Germany, and forms one of the staff of the Würzburg University, which numbers amongst its professors Scanzoni, Kölliker, Heinrich Müller—names already well known to the British medical public. Many of our readers will, no doubt, remember him while residing in Dublin, and recollect his admirable descriptions, in a German medical periodical, of the Dublin School of Medicine and its representatives, towards whom he expresses a lively sense of gratitude for the kindness shown him, and the instruction imparted to him. Those who are acquainted with the writings of our best aural surgeons will not find very much that is new to them in these lectures; but they will find them thoroughly practical, and containing much valuable information on pathological subjects, as well as on the state of aural surgery on the continent. Compared with other German publications on the same speciality, this work ranks very high, and is characterized by the absence of many unscientific and untrue doctrines.

The Renewal of Life; Clinical Lectures, Illustrative of the Restorative System of Medicine, given at St. Mary's Hospital. By THOMAS K. CHAMBERS, M.D., &c., &c. London: J. Churchill. 1862. Pp. 430.

THERE are two classes of medical works to be met with daily in general circulation—the strictly professional, addressed solely to

the profession, and the pseudo-professional, addressed to the public in general, and to hypochondriacs in particular. When we chance to meet such as the latter, we either pass them by or punish them, as we deem it our duty to endeavour, by all means in our power, to discourage such productions by exposing them. We, however, meet sometimes, though rarely, we are thankful to say, with books partaking of both characteristics—books scientific, with a popular, catching title, which will attract the unprofessional reader, even more than the professional student. When dealing with such we naturally find ourselves rather in a difficult position; we may fully approve of the subject matter, at the same time that we heartily condemn the way it is presented; we have, however, no choice; our duty is plain, and however painful such duty, we shall always do it unflinchingly.

The book now before us seemingly belongs to this latter class, it consists of a series of clinical lectures, and fragments of lectures, delivered at different times, upon different subjects; we find much in it that we approve of, much that we utterly condemn, and we regret it extremely, for our pen can as easily trace words of praise as words of blame; and it is far pleasanter to us to write in the former than in the latter strain. To begin, then, we object to the title; why not call these 430 pages "Clinical Lectures delivered at St. Mary's Hospital, by T. K. Chambers, M.D."? Such title would have been amply sufficient for the professional man or the medical student, though it would just as certainly have failed to attract the unprofessional eye. But this was evidently what Dr. Chambers was aiming at, when he selected, as a title to his book, "The Renewal of Life!" Who would not renew his life if he could, though, perhaps, he would not like to live it over again? Which of us, on the shady side of remorseless 40, would not again renew his life, and with it the vigour of his mind and body? The hard-working lawyer, the anxious and care-worn merchant, the dyspeptic alderman, the gouty peer, the shattered man about town, all would hail with joy a book that discoursed about "The Renewal of Life," and all would naturally seek out the doctor who, having written such a book, would certainly, in their minds, be able to carry out his principles, and work upon their frames that blessed renovation.

The book is divided into twenty-nine chapters, of which we shall now proceed to give a short analysis. The first treats of "Theories of Medicine at the Bedside;" of these the author says there are five, which he enumerates as follows:—ALLOPATHY,

HOMEOPATHY, EVACUATION, COUNTER-IRRITATION, and STIMULATION, to which is added, at the end of the chapter, his own special theory, RESTORATIVE MEDICINE. After giving an account of the first five theories, he adds, "in fine, all confess, either by words or deeds, that a true guiding principle, a single aim, in the treatment of disease, is wanting." . . . "It is no use to mince the matter, the root of the whole principle is unsound." We hardly know how to deal with such expressions and assertions. Who are the allopaths? If Dr. Chambers means such men as Graves, Todd, Watson, or Stokes, he is decidedly wrong, and what is more, grossly ignorant of their views and practice. Allopathy was a nick-name given by the homeopaths to the followers of rational medicine, and not one which they assumed themselves; and Dr. Chambers ought to know, that we are not guided in our practice by the "simple and very plausible idea, that the important point in all diseases is a motive cause affecting the body with a certain derangement, and it suggests that the best mode of combating that, is to give remedies whose nature is to affect the body with an opposite derangement. Thus a balance is struck between disease and remedy. If there be constipation, drugs are to be given, which, to a healthy person, would cause diarrhea; if the urine be scanty, those which normally augment it beyond the average standard. . . . . In short, all states of excess are to be cured by the opposite excess; the disorder is to be *neutralized* by the temporary presence of a contrary disorder." All this is simply untrue; but Dr. Chambers required an excuse for putting forward his own RESTORATIVE doctrine, and this excuse he makes for himself, by concocting a mass of rubbish and calling it "Theories of Medicine at the Bedside." We shall now endeavour to analyze for our readers this new doctrine of the author.

Having devoted the second chapter to the consideration of DEATH, LIFE, DISEASE, and CURE—in which the student will find many truths, which he will equally meet in other works on the practice of medicine—Dr. Chambers plunges boldly into "Continued Low Fever," and, after switching the allopaths, brings himself prominently forward thus:—"Here then we bring our doctrine of RESTORATIVE MEDICINE (the capital letters are Dr. Chambers') to its touch-stone—bedside application. The Restoratist asks himself what vitalities are wanting, and where?—and how shall I easiest supply them?" Read two and a-half pages more, anxious student, and you will find the answer to be—sour buttermilk and

leeches to the right iliac fossa! The chapter on rheumatic fever is good, though blistering the painful joints would, in our mind, be a better treatment than leeching them. In pneumonia Dr. Chambers advocates bleeding—either leeches, cupping, or from the arm; and although we do not object to taking away a little blood *locally*, when it acts as an *obstacle*, yet we fear that he is too partial to detraction of blood, for at page 117 we read, "I have called the local detraction of blood a renewal of life;" and at page 171, "You need not be afraid of a small loss of blood in low fever;" though we must do him justice by saying, that whenever he does detract blood he does not follow it up by low diet, but, on the contrary, always supports the system.

The fourteenth chapter treats of Thoracic Aneurisms, and here again his partiality for blood-letting is apparent. "The treatment which is adopted is a strict following of that which nature adopts in all aneurisms that heal spontaneously. In them you find a fibrinous clot formed, and the sac shrunken up from lack of a stream passing through it. We have endeavoured to make the blood disposed to form fibrinous clots, by bringing it into that fibrinous state, which loss of blood, anemia, and low diet induce; and we have endeavoured to keep the blood-stream as calm as possible by rest in the horizontal posture." . . . . "I like, in general, to bleed the patients from time to time." . . . . "I kept a stone-mason in this hospital under the bleeding and starving treatment for five weeks." . . . . "Bleeding and starving, it is urged, induce debility and anemia; that is to say, in physiological language, they lower the force of the heart, and they cause a comparative excess of fibrin over blood-discs in the blood. These are both steps towards the mode adopted in nature to effect a cure of aneurism, and perhaps of some other diseases also. So I contend not merely is it the best mode, but the only honest mode of treating aneurism of the trunk-vessels; because it is the only one we at present know consonant to reason and experience." How such treatment can, by any stretch of fancy, be called restorative we can not possibly make out; but surely the author does not mean to tell us that it is new, and originates with himself? It is simply old Valsalva's plan, practised so long as a hundred and fifty years ago; and in lecturing to his class Dr. Chambers should not have allowed the opportunity to slip of giving credit to the learned Italian physician, who, in those days of medical infancy, did so much for the advancement of real science. But even the aid of the alkaline treatment which our author recommends at

p. 225 (3 drachms daily of liquor potassæ) is not new; Sir Astley Cooper employed it—and in his lectures cautions the reader against the protracted use of it, on account of its producing petechiæ.

The seventeenth chapter treats of Hysteria; and from it we learn what we certainly never knew before—that the air of Rome has a remarkable tendency to the production of it; and we find at page 256 the following wholesome advice:—"Be careful, therefore, that your hysterical patients keep clear of the Eternal City." The treatment recommended consists in "moving the limbs to time, at first slowly, and afterwards more rapidly, till at last the culminating point of dancing can be arrived at. This is the perfection of the cure." We shall insert one more extract, and then put by our pen, for we have already exceeded our limits. At page 361, when treating of diarrhea, Dr. Chambers speaks as follows:—"In the case of babies, the whole abdomen and loins may be fastened up in a large circumambient poultice, which they cannot wriggle away from; one or two leeches put on near the navel, and the bites allowed to bleed for some time." *One or two leeches applied to a baby's abdomen and then allowed to bleed into a huge poultice!* We fear our readers will scarcely believe us, but it is the fact; they will find it written at p. 361; and surely a safer passport to the regions of bliss could not have been devised for any baby!

Recherches d'Anatomie et de Physiologie Expérimentale, sur les voies Lacrymales. Par le Docteur Foltz, *Professeur d'Anatomie, et de Physiologie à l'Ecole de Médecine de Lyon.*

Recherches sur l'Anatomie, et la Physiologie de la Conjonctive. Par le Professeur W. Krause.

The first of these papers treats of the anatomy and physiology (based on experiments) of the lachrymal canals in man and other mammals, and consists of three parts; one on the anatomy of the lachrymal canals; one on the physiology of the canals; and one giving a historical and critical review of the various theories that have been broached on the subject of the course of the tears. We purpose to give a brief epitome of each of these.

Dr. Foltz first examines the lachrymal canals in man, dividing them naturally, into two parts, very different both as to their

structure and the mechanism of their functions—the lachrymal canals and the lachrymo-nasal canal.

The first question of importance to determine is, whether the lachrymal canals are *open* or not; the majority of anatomists compare them to capillary tubes, and in consequence regard them as both cylindrical and open; but in opposition to this Béraud[a] says that they are closed in the same manner as the œsophagus is; and Dr. Foltz himself, in a paper published in the *Medical Gazette of Lyons*, in 1860, also participated in this view; more recent observations have, however, convinced him that they are neither cylindrical nor shut, but that they are of an oval form and open.

In the first part of the canal, and about a millimètre and a-half from the punctum lacrymale, the canal, from having been a little enlarged, presents a very remarkable semicircular contraction, which is a true valve, the free edge of which is turned towards the lachrymal sac; in the second part there are sometimes found a few slight folds, and some very minute *culs-de-sac;* in a few rare cases these folds have been found large enough to form valves, in all respects resembling those of the lymphatic system; in the third portion there is nothing very remarkable to record, unless that the valve of Huschke appears to be constantly present, though rather various in form.

Of the two valves in the lachrymo-nasal canal, the inferior valve appears to be the more complete and the more constant, although very often wanting—according to Béraud three or four times in a hundred, and according to Sabatier and Foltz even as often as eighteen times in a hundred.

We shall pass over very briefly the account of the lachrymal canals as found in the horse, the rabbit, and the hare—not from any want of appreciation of their interest, but fearful lest we should tire the reader by dilating too much on the comparative anatomy of these little passages. It may be well, perhaps, to state that, in the horse only one of the three portions found in the canaliculi lacrymales of man exists; and this corresponds to the middle portion in man; while in the rabbit and hare there is but one lachrymal canal and punctum for each eye (two puncta have been found on one papilla of the human eye), the punctum of which is found on the border of the inferior eyelid; the canal is of large calibre, and there can be no doubt as to its orifice being constantly patent.

Dr. Foltz next proceeds to detail the results of his experiments, of which the following are the most remarkable:—On a sufficiently

[a] Velpeau and Béraud, Manuel d'Anat. Chirurgicale. 1862, p. 106.

large rabbit, Dr. Foltz made a lachrymal fistula by cutting out, with a scissors, a small portion of the skin of the orbicularis, and of the external walls of the canal; nothing was easier than to be certain as to the openness of the very large canal; a drop of water, coloured by indigo, was placed between the eyelids, absorption took place rapidly, and the blue colour appeared in the fistula. When the animal winked its eyelids the liquid in the canal was ejected from the fistula; when the winking ceased, and the eyelids were averted, the liquid re-entered, by a true suction, through the fistulous opening; during many days the animal was watched, and the same results were obtained; different substances were tried—among them ink, this considerably increased the secretion of the tears, which were immediately absorbed, and appeared of a black colour at the fistula. A glass tube was then fitted to the fistula, and during each contraction of the eyelids the liquid passed through the tube, coming back again during their relaxation. This remarkable escape of the liquid through the fistula was produced solely by the contraction of the upper and lower eyelids, but it took place much more energetically during the winking of the nictitating membrane.

From these experiments it is evident that during the winking of the eyelids, and especially during that of the third eyelid, the lachrymal canal is compressed, and expels onwards the fluid which it contains; and that when the eyelids open to return to their normal position, the canals recover their capacity, and take in the tears through their puncta. Dr. Foltz had started with an idea the very reverse of this, fancying that it was the winking that dilated the passages, and the opening of the eyelids that compressed them. These experiments obliged him, however, to recognise, on the contrary, that there is a kind of forcing-pump movement during the winking of the lids, and that of a suction-pump during their relaxation. This theory of the course of the tears having been determined by these and some additional experiments, it remained to be found out how much is due, in the compression of the lachrymal canals, to the action of each eyelid, or rather to the muscles which move them; these muscles being for the eyelids, properly so called, the orbicularis, and for the nictitating membrane, the retractor muscle; the former being supplied by the facial nerve, and the latter by a branch from the sixth.[a] To do this it was

[a] This membrane is commonly supposed to be supplied by a branch of the third nerve, but the experimental physiologist, Chauveau, has determined its supply as here given.

necessary, first, to cut these nerves, and to observe the effect of their destruction on the mechanism of the lachrymal canals, and on the other hand, to excite them by means of galvanism, and likewise to note down its effects.

We have not room to give these experiments in detail; Dr. Foltz was assisted in them by Professor Chauveau, but the conclusions are too interesting to be omitted. In a rabbit the facial was cut, and it was found that the orbicularis contributed but very feebly in assisting the onward progress of the tears; but in another rabbit the sixth was cut across, and it was proved that the nictitating membrane plays the principal part in the mechanism of their progression; lastly, both nerves were cut across, and there then followed a complete paralysis of the three eyelids, so that coloured liquids placed between the lids remained stationary on the lower one; a fistula was then established, but the blue coloured liquid did not arrive at the fistula, although the punctum lacrymale and the canal remained open, and on the next day both fistula and canal were thoroughly dry, not a drop of liquid having passed, showing clearly enough that the tears do not escape into the nasal sac by means of capillary attraction.

The following are the final conclusions of the author:—1st. That in mammals like the rabbit and horse, the lachrymal canals are the seat of two passive movements, the one a systole, the other a diastole, coinciding exactly, the first with palpebral winking, the second with the opening of the eyelids. 2nd. That the systole consists of a lateral flattening of the sides of the canal, which goes on until its calibre is completely obliterated. 3rd. That two muscles contribute to this movement—the orbicularis, which directly compresses the external wall of the canal, and the retractor muscle, a portion of which compresses the internal wall. 4th. That the retractor muscle of the eye, supplied by the sixth nerve, has a much more energetic action on the lachrymal canals than the orbicularis, which is supplied by the facial. 5th. That during the palpebral winking and the systole of the canal, the orifice of the punctum lacrymale is shut by a peculiar valvular mechanism, the anterior and posterior edges of the slit approach close to each other, and the internal and external walls apply themselves the one to the other, so that the tears compressed in the canal, and not able to get out through the punctum, must issue out into the nasal sac. 6th. The diastole restores the canals to their normal open condition, in virtue of their peculiar elasticity, and of the return of the organs to their

wonted position. 7th. During the diastole the punctum lacrymale opens, the tears assembled in the lake are sucked into the canal. It may be asked why, in animals which have no valve at the end of the lachrymo-nasal canal, the tears do not flow back again into the canal? There are many reasons why they cannot, such as the great length of the canal, its being surrounded by bone during part of its course, and its structure being submucous towards its termination allows its walls to yield as a valve; but the principal obstacle is the peculiar action of Harder's gland, which ceases not to compress the whole canal. When it, *i.e.*, Harder's gland, is withdrawn to take its place in the interior part of the orbit, it quickly withdraws from pressing upon the punctum lacrymale, and then it gradually ceases to compress the rest of the canal; the part close to the punctum ceases, in the first instance, from being compressed, and sucks in the tears before the compression has been removed from the point of exit. In a strong rabbit, in which the external wall of the canal had been removed, one could follow with the eye the movement of the gland as it pressed upon the interior wall. 8th. Once the tears have arrived in the lachrymo-nasal canal, they run out in virtue of a force, *vis-à-tergo*, in addition to the force of gravity, and perhaps even that of respiration.

It only remains now to see how these facts, ascertained by experiment, are applicable to man.

It is evident, that in man, as in the other mammals, the lachrymal canals are the seat of two passive movements; one, a systole, coinciding with the winking of the eyelids; the other, a diastole, coinciding with the opening of the lids.

The systole consists of an antero-posterior approximation of the canals, attended by a slight shortening, and a lateral inflexion. This is produced by the orbicularis and Horner's muscle, which form two planes of contraction between which the canals find themselves compressed in the instant of winking.

The diastole results, in man as in the other mammals, from the elasticity of the canals, and the restoration of the organs to their wonted position.

During the palpebral systole the punctum lacrymale is closed. It is easy to ascertain, by the assistance of a good lens, that its orifice, in other respects a very narrow one, closes itself by a flattening from without and within. Moreover, Dr. Foltz has pointed out, in the first part of the canal, a millimètre and a-half from

the punctum, the existence of a valve, which assists in narrowing the tube, and which opposes the reflux of the tears, during the compression of the canal. This valve, though insufficient during the diastole of the canal, is quite sufficient during the systole which contracts it; hence, it follows, that the tears compressed in the canals, get heaped up against the valve and its *cul-de-sac*, and so, not obtaining any exit on that side, are projected into the lachrymal sac.

During the diastole the punctumlacrymale opens itself, and lets the tears pass in; it is to be recollected that the valve of Huschke hinders the tears from getting from the sac back into the canal.

The lachrymal canals in man, thus furnished with two valves, the one at their entrance, the other at their exit, are infinitely better organized, than those of other animals, to play the part of a forcing and suction-pump.

The question, therefore, of whether the sac is *compressed* or *dilated* during the palpebral winking, and on which so much has been written, in support of preconceived ideas, is a question of but second-rate importance. We have seen that the rabbit wants this sac; that in the horse it is fully developed, and very slightly compressed, although the canals are very strongly so. Hence, by analogy, we may conclude, that in man the sac is submitted to a slight compression during the winking of the eyelids; but the important fact is, the systole of the canals themselves, once the tears get into the sac, they run along through the lacrymo-nasal canal, partly by the *vis-à-tergo*, and partly by gravitation. It may be added, that the act of suction is not, perhaps, entirely absent in the sac and nasal canal, for it happens with many persons that a strong inspiration—the mouth and the nose being held shut—depresses the sac, while a strong expiration, under the same conditions, makes it pout out. This effect of respiration, which does not exist in every person, can only affect the tears in the lacrymo-nasal canal, and not those in the lachrymal canals.

So far for the first and second portions of Dr. Foltz's paper. In the third he proceeds to enumerate in detail the different theories that, from time to time, have been started on this subject. The author remarks, justly enough, that none of them have been founded on experiment, but are simply the result of anatomical or pathological inductions. Commencing with Petit (1726?) who compared the lachrymal canals to a syphon, of which the short branch is represented by the lachrymal canals, and the long by the sac and

nasal canal; as a syphon, however, must be charged, Petit thought that the contraction of the eyelids, in compressing the tears in the sac, compelled them to enter into the lachrymal canals. This ingenious theory of the syphon would not, however, do; it condemned the most dependent of the eyes, when their possessor was lying on his side, to weep; and although there was obstruction of the nasal canal, still the sac filled with tears; and, lastly, the syphon theory did not explain the movements of systole and diastole.

Haller (1763), Molinelli and Magendie (1833), believed that the tears penetrated the canals by means of capillary attraction.

Some ten years later Haller held somewhat the same opinion as Richerand (1802), that the tears were sucked in by an action *vitale particulière.*

We pass by the opinion of Weber, Hasner, Janin, in order to allude to the opinions of Hyrtl and Arlt. The first bases his theories on the *dilatation* of the sac; while Arlt's theory is founded on its *compression.* Hyrtl in his *Topographical Anatomy*, first published in 1847, compares the action of the lachrymal canals to that of a suction-pump, each winking being, as it were, the stroke of the piston.

Arlt, presuming that the lachrymal passages are always filled with tears, says—suppose that at the moment of the shutting of the eyelids, the sac and lachrymal canals are somewhat compressed by the swelling out of the contracted fibres of the orbicularis, and by the traction which at the same time Horner's muscle exerts from behind; then the walls of the sac are so resisting, and its contents so incompressible, that only a certain portion of these can be pressed out, and these will naturally go by the way which offers either no obstacle, or else offers obstacles relatively much less than the other; this latter condition is found to exist on that side of the nasal canal in which the tears pass, and from which they tumble drop by drop into the nasal fossa. At the moment when the eyelids open themselves the compression of the nasal sac immediately ceases; this cavity assumes its ordinary dimensions, and the tears of the lachrymal sac will necessarily penetrate into the space left empty.

The principal argument used by Arlt to sustain this theory, is derived from an examination of lachrymal fistulæ; he has remarked, that the liquid which may be seen at the orifice of a fistula leaves it at the moment of the eyes winking, and comes back again when they re-open; hence he has presumed that the compression of the sac was due to the palpebral winking.

Many objections have been raised against this theory; among others, in conformity with it, it is necessary to believe that the sac and canal should be *completely* filled with liquid, for if it were not so, then, when the compression of the sac would cease, the tears would not fail to flow all back again. M. Sabatier asserts too that this complete fulness of the canals is by no means generally the case, but, on the contrary, they are often not more than half full. Another objection is based on the necessity of a valve at the inferior end of the canal, though the presence of this is by no means constant; and another still graver objection is, how is it possible to understand why, with a large artificial opening into the sac, all epiphora should cease, when it is then it really ought to be produced.

It has been already seen how, that by substituting the compression of the lachrymal canals for that of the lachrymal sac, all these difficulties are not only completely got over, but that, in addition, the facts acquired by a knowledge of pathology or anatomy will then be in accord with those derived from experiment.

We can recommend the perusal of this paper by Dr. Foltz to the reader; the foregoing very short notice is but a very brief outline, indeed, of some twenty pages of interesting details. We have endeavoured to allow the author to speak for himself in the epitome of his paper given, and have refrained from drawing therefrom any inferences. We cannot, however, be sufficiently thankful to the experimental physiologists who help thus to place on a more sure basis the functions of organized beings

The next paper is one on the anatomy and physiology of the conjunctiva, by Professor W. Krause; it was written for a work by Dr. Welcker (*Etudes Opthalmologiques*), of which the first portion has appeared, and it thus reminds us that some very interesting contributions to the anatomy and physiology of the eye have lately made their appearance in connexion with works on eye surgery; such as Ch. Robin's chapter on the pathological anatomy of cataract, in *Dr. Desmarres' Diseases of the Eye;* and the epitomized chapter on phosphène, by Dr. Serre D'Uzes, in *Warlomont and Testelin's Translation of Mackenzie*, and others.

The author calls attention to the conjunctiva as consisting of two several portions; one the mucous layer, composed of epithelium, the other the papillary layer which is connected to the deeper layers of the globe; the connexion varying in different parts of the eye; it is

formed of a uniform layer of compact cellular tissue, which, on the lid near the eyelashes, presents the appearance of very small, scarcely visible, papillæ; towards the posterior portion of the tarsal cartilage, they become more prominent, and are found in the form of tongue-shaped projections; in the conjunctiva of the *cul-de-sac* they have a larger base, but are less elevated; these papillæ are formed of a cellular tissue, with very solid nuclei; they are well supplied with blood vessels, but it is uncertain whether they possess lymphatic vessels; their size varies according to the age of the individual, and according to the different parts of the conjunctiva in which they are found; lymphatic vessels are very common in the the conjunctiva of the bulb, and at the border of the cornea they form a net-work with close meshes, a full description of which is given.

Lymphatic glands are also found in the conjunctiva, immediately beneath the mucous surface; they are scattered here and there in the mucous membrane of the *cul-de-sac*, also in the inner half of both eyelids; their number is very variable, sometimes not one is to be found: they resemble in every respect the solitary glands in the intestines, being, like them, in communication with lymphatic vessels.

The nerves of the conjunctiva are said to terminate, not by losing themselves in the tissue, but in club-shaped corpuscles, such as have been observed in the tongue, lips, clitoris, and glans penis, and which bear some resemblance to the middle portion of a Pacinian corpuscle.

The aggregated glands are principally found in the upper *cul-de-sac;* ordinarily there are twelve to eighteen (once forty-two were seen) in the upper *cul-de-sac*, while there are not more than two to six in the lower; each gland or cluster of glands has its own duct; these glands were discovered in 1842, by M. C. Krause; Dr. W. Krause thinks that they do not secrete mucous, but tears, and this on account of their nearness to, and anatomical accordance with, the lobules of the lachrymal gland itself.

The watery fluid which accumulates in the cavity of the sac of the conjunctiva is composed of the product of the secretion of the lachrymal gland, of the secretion of these glands, as well as that of the vessels of the conjunctiva and cornea, and is mixed up with the detritus of the epithelium lining these membranes.

The secretion of the Meibomian glands appears to have no other office than to lubricate the eyelashes, and to prevent the tears from running over on the cheeks; it is composed altogether of fat.

The tears, which comprise as well the secretions of the conjunctiva,

form a clear colourless liquid of a saltish taste, containing according to Frerichs:—

	I.	II.
Water,	99·06	98·70
Solid Matter,	0·94	1·30
	100·00	100·00
Solid Matter:—		
Epithelium,	0·14	0·32
Albumen,	0·08	0·10
Chloride of sodium, alkaline phosphates, earthy phosphates, fat and extractive matter,	0·72	0·88
	0·94	1·30

The first analysis was made on tears which had been obtained from a healthy eye by irritating it with an electro-magnetical machine; the second, on tears from an eye affected with chronic ophthalmia.

As the epithelial cells become decomposed on the surface of the conjunctiva, they form a viscid fluid, mixed with little grains of amorphous matter which contains (in part only) a little fat, and which has been, and is often, called the mucous secretion of the conjunctiva; this collects in the corners of the eye, and has often a frothy appearance.

The researches of MM. Martini and Frerichs, as well as the surgical observations of Textor, Bernard, and others, prove that the lachrymal gland is not the sole source of the quantity of the fluid which flows over the eye; for though after extirpation of the lachrymal gland the quantity of the fluid is much diminished, yet the eye can keep its brilliancy and preserve a certain degree of humidity.

This paper is illustrated by a plate representing the termination of the nerves in the claviform corpuscles, and of the papillæ which project up into the epithelial layer of the conjunctiva.

PART III.

MEDICAL MISCELLANY.

Reports, Retrospects, and Scientific Intelligence.

PROCEEDINGS OF THE PATHOLOGICAL SOCIETY OF DUBLIN.[a]

TWENTY-FIFTH ANNUAL SESSION, 1862-63.

MR. HAMILTON, President.

Diphtheria.—DR. EDWARD HAMILTON exhibited the larynx of a female about seventeen years of age. She was admitted into Steevens' Hospital on Sunday evening last, and he saw, during his ordinary visit on the following day, she was suffering from all the symptoms of sore throat in its most aggravated form; there were great swelling, difficulty in swallowing, and tenderness on pressure outside. The lower part of the uvula had a black appearance; the tonsils were enlarged, and covered with a slough, which extended posteriorly for some distance. There was no evidence of laryngeal distress, and her breathing was tranquil. The usual remedies were employed, and she appeared to improve somewhat under the treatment, and the difficulty in swallowing seemed to subside. Up to this time there was no laryngeal distress, but on Tuesday there were uneasiness and signs of anxiety; she passed a restless night, and on Wednesday morning there were signs of laryngeal inflammation beyond all doubt, her breathing was more or less affected, and her lips blue. A consultation was then held upon the case, and it was determined to perform tracheotomy, which was accordingly done at once.

The laryngeal symptoms were much relieved by the operation, and the pulse increased in force. Things went on in this way for a few hours, but, the pulse failing, she died at 12 o'clock, eight hours after the operation.

The *post-mortem* examination showed the epiglottis covered with a tough yellow exudation, extending through the larynx, covering the

[a] These reports are furnished by Dr. R. W. Smith, Secretary to the Society.

chordæ vocales (although there was no loss of voice), and extending for a considerable way into the trachea, at one point presenting the appearance of slough described by Empis, resulting from chemical change in the exudation; but on raising this the mucous membrane was seen to be quite sound.—*November* 29, 1862.

Hypertrophy of the Heart—Adherent Pericardium. Dr. Banks detailed the following case:—

A boy aged 18 was admitted into the Hardwicke Hospital complaining of pain, of extreme severity, in the back. He stated that two years previously he had rheumatic fever, and that he was told his heart was affected. His health, however, was good, and he did not seem to have suffered any inconvenience from palpitation or shortness of breath, as he was capable of very active exertion and hard work.

He had recently come from the country, and for some time had failed to obtain employment, being subject during this period to much privation.

At length he succeeded in procuring a place, but he was obliged to work in a cellar, and he soon got cold from wet feet.

Before he was received into hospital he had been some days ill, but when scarcely able to leave his bed he was compelled to perform his arduous duties.

On the first day he came into hospital he exhibited febrile symptoms, and complained of excruciating pain along the spine. On the following day there was slight swelling of the knee joints.

The next morning (the third day of his residence in the hospital) his countenance was indicative of great distress, and he referred all his pain and anguish to the region of the heart, over which he could not endure even the slightest pressure. The heart's action was tumultuous, and the impulse was visible over an extraordinary extent.

The whole precordial region was dull, a loud murmur was audible at the apex; there was remarkable throbbing of the carotids. The pulse was full and perfectly regular, but rose in frequency for the three days he lived after the supervention of the cardiac symptoms, until shortly before his death, when it fell from 130 to 70; death was sudden.

A *post-mortem* examination revealed the following morbid condition of the heart:—

Enormous enlargement, its weight being 2 lbs. 6 ozs. The pericardium universally and closely adherent to the heart's surface; much of the uniting material (lymph originally) was found converted into fatty matter.

Whilst all parts of the heart were enlarged, the immense size the organ had attained was chiefly due to hypertrophy and dilatation of the left ventricle; this cavity was of a circular form, having a transverse diameter of at least five inches; and its walls, in parts, were nearly two

inches in thickness. The right ventricle, though dwarfed and small as compared with the left, was, in point of fact, much larger than natural, and its walls thicker.

The sigmoid valves of the aorta were found permanently patent; this was owing to dilatation of the orifice of the vessel, for the valves themselves were larger than the normal size, and were greatly thickened, yet they were insufficient, for water flowed freely from the aorta into the ventricle.

The mitral valve presented a similar appearance; the curtains were increased in size, and much thickened; on the anterior flap a granular appearance, due to recent deposit of lymph, was recognisable, with patches of well marked vascularity, unaffected by frequent washing.

The chordæ tendineæ were greatly hypertrophied, and the left auriculo-ventricular opening was dilated. The auricles were more capacious than natural, and fibrinous coagula occupied both right and left cavities, extending on the right side into the superior vena cava, and on the left into the aorta; the latter presented an accurate cast of the aortic valves.

Lastly, the heart's substance was undergoing fatty transformation, from without inwards.

In some places, as near the apex of the left ventricle, this fatty conversion extended very deeply into the muscular tissue; at the same time there were no evidences, on microscopic examination, of true fatty degeneration of the muscular fibre.—*December* 6, 1862.

Aneurism of the Aorta.—Mr. Symes exhibited the heart, lungs, and aorta, taken from the body of a constable, who died suddenly in the Phenix Park. On examination after death, the left pleural cavity was found to be filled with blood. Atheromatous deposits had occurred in many parts of the aorta, and there was also a large aneurism springing from the inferior wall of the transverse portion of the arch; it was imbedded in the substance of the left lung. The rent, through which the fatal hemorrhage had taken place, was in the left wall of the tumour; through this the blood passed into the substance of the lung, and making its way to the anterior aspect of the organ, burst through the pulmonary pleura, into the serous cavity.

Though this man had been under treatment, in the country, for debility, the existence of the aneurism had not been ascertained during life, owing, no doubt, to its being surrounded by pulmonary structure, a circumstance calculated to render the diagnosis extremely difficult. The case was worthy of attention from the peculiar situation of the tumour.—*December* 6, 1862.

Fracture of the Humerus.—Mr. Smyly, sen., presented a specimen of comminuted fracture of the anatomical neck of the left humerus.

The case was that of a man aged 70 years, a slater, who fell from the top of a house three stories high, along with the ladder on which he was standing. There was no parapet to the house; the ladder, on which the patient was working, was secured to another which hung upon the roof at the other side; unfortunately the rope was rotten, and gave way, and the old man was precipitated to the ground, and fell upon his left side. He was immediately brought to the Meath Hospital; and, as it was supposed that the shoulder was dislocated, Mr. S. was sent for, and found that the humerus was fractured close to the shoulder joint; that the left upper eyelid was split, and the left knee was contused, and the skin abraded. The man was perfectly conscious; on the fourth day he had an attack of erysipelas of the face, commencing at the wound in the eyelid. On the fourteenth day typhoid fever supervened, with pneumonia, and the patient died on the eighteenth day after the accident, of pyemia.—*December* 6, 1862.

Disease of the Prostate Gland.—Dr. Barton exhibited the bladder, prostate, and urethra, taken from the body of a man, about 65 years of age, who died in the Adelaide Hospital some time before. The prostate was the size of a small orange, its central portion projecting an inch into the bladder; it presented the usual hard, fibrous feel of chronic enlargement. The effects of the chronic enlargement of the prostate were remarkably exemplified by this specimen.

1st. The bladder was small, its mucous membrane of a dark green colour, its walls very much hypertrophied, the muscular bands presenting the appearance of the carneæ columnæ of the heart.

2nd. The ureters were dilated to about four times the natural calibre.

3rd. The kidneys presented an amount of disease which was undoubtedly the immediate cause of death. Their cortical substance was filled by small purulent deposits, while the lining membrane of the pelvis and calices was of a dark red colour. All these changes followed upon the obstruction at the neck of the bladder, caused by the enlarged prostate.

The symptoms were those invariably present in such cases. For some weeks before the death of the patient, he was obliged to have the catheter passed every four or five hours. For three days previous to his death, he became heavy and drowsy, and finally comatose for 12 hours before death. The state of the kidneys explained this.

A remarkably large sacculus existed to the left side of the bladder, capable of holding a billiard ball, and communicating with the bladder by a narrow neck, the mucous membrane of the sacculus and of the bladder being evidently continuous.

It had been said that such a condition might be diagnosed by a quantity of pus and mucous *following* the discharge of the urine contained in the bladder. The symptom certainly was present in this case; but Dr.

Barton had supposed the mucous which was expelled after the flow of urine, came from the *bas-fond* of the bladder.—*December* 13, 1862.

Hematocele of the Tunica Vaginalis.—Professor R. W. Smith exhibited a specimen of this disease, which had been removed on the 26th of last month. The patient, who was about 45 years of age, and of a pale unhealthy aspect, was admitted into the Richmond Hospital on November 20th, under the care of Dr. Hutton, with a very large tumour, occupying the right side of the scrotum. He stated that, about three years ago, he was on the top of a load of hay, jumping on it, on his knees, in order to press it down, and that while doing so he hurt his right testicle, but not so severely as to cause him much annoyance. Three months subsequently, however, his attention was attracted by a tumour occupying the right side of the testicle; it was not attended by pain, but slowly and steadily increased until the last year, when its growth became much more rapid, and when the man was admitted the tumour was as large as a cocoa-nut. Within two months previous to his admission it had been tapped three times; between the first and second tapping there was only an interval of four days; the operation was performed, for the third time, ten days before his admission into the hospital; upon each occasion a large quantity of a grumous, bloody fluid was evacuated, but (and it is a circumstance worthy of note) without producing a corresponding diminution in the size of the tumour.

When he was admitted, the tumour (as already mentioned) was as large as a cocoa-nut; in form it somewhat resembled a hydrocele, but was more globular; it was very heavy, and had a feeling of elastic solidity; the scrotum was tense and red, but there were not many veins to be seen upon its surface; the tumour was everywhere perfectly opaque; the cord was healthy; and there was no glandular disease; the testis could not be felt. Considerable doubts were entertained as to the exact nature of the tumour, for, although its mode of origin and the result of tapping pointed to hematocele, there were other circumstances calculated to excite the suspicion of its being a malignant growth; for instance, the great weight of the tumour, its firmness and solidity, its elastic feel, the impossibility of distinguishing the testicle, and the very unhealthy aspect of the patient. Professor Smith observed, that although he inclined to the opinion that the case was one of hematocele, yet he had fully concurred in the propriety of performing castration. The operation was done by Dr. Hutton, and the patient made a rapid recovery, without the occurrence of any untoward symptom.

Upon making a longitudinal section of the tumour, the tunica vaginalis was found to be greatly diseased, immensely thickened, and possessing the firmness of cartilage; it varied from a quarter to half an inch in thickness; its interior contained masses of coagulated blood and a thick

grumous fluid composed seemingly of purulent matter and decomposed blood. The testis was completely atrophied.—*December* 13, 1862.

Tubercular Peritonitis.—Dr. Mapother brought forward a case of tubercular peritonitis. The patient, a female, 23 years of age, had been for many months under the observation of Dr. O'Ferrall, and suffered all through that period from considerable pain in the abdomen. Vomiting was the most marked symptom. Some time before her death the intensity of the pain increased, the abdomen became tympanitic, and the vomiting, which had been previously of a bilious character, became like feculent matter, and she sank quietly in about five days afterwards. On making a *post-mortem* examination, it was found difficult to remove the viscera of the abdomen, as they were agglutinated by lymph. The very small cavity which remained was filled with feculent matter, similar to what she vomited. It was also difficult to remove the liver, in consequence of the adhesions to the diaphragm. The stomach presented a healthy aspect internally, but externally was most firmly glued to the liver, omentum, and abdominal parietes. This condition may account for the constant vomiting, as the organ could not contract on its contents, yet they might be ejected by the pressure of the abdominal muscles. In the last four feet of the ileum six perforations were found, presenting ragged surfaces towards the mucous membrane, and smaller openings in the peritoneum. There were four or five tubercular masses in each ovary, and a very limited deposition in the apex of the left lung.—*December* 13, 1862.

Cystic Disease of the Testicle.—Dr. J. S. Hughes said:—The recent specimen which I have to submit to the notice of the society consists of an enormously large and diseased testicle, which I removed from a patient, aged 24, on the 15th of last month, in Jervis-street Hospital.

The history of the case was as follows:—In August, 1861, whilst serving in the Military Train, the patient was thrown on the pommel of his saddle, on his right testicle, which at once became painful and swollen. The pain soon subsided, but the swelling continued to increase until the date of his discharge from the service in November of the same year.

In May last the swelling sensibly increased, but he was able to work as a railway porter, suffering little inconvenience, except from the size of the tumour, until, in play, he received a kick in the diseased organ from a fellow-workman, after which it became painful and more enlarged; and in the latter end of September the scrotum gave way at the precise spot which had been struck.

He was admitted into Jervis-street Hospital on the 6th of last month. The testicle was of great size, and heavy; it measured in the vertical diameter eight inches, in the transverse nine and a-half inches; it was

of stony hardness in some places, in others tense and elastic, and yielded an indistinct sensation of fluctuation. A large slough, which emitted a fetid odour, occupied the anterior inferior aspect of the tumour. There was no pain complained of, nor was there any disease of the spermatic cord or inguinal glands. The scrotum was of a dark livid hue, the cutaneous veins turgid and varicose, and the integuments of the penis were involved in the diseased mass; the left testicle was pushed upwards towards the left inguinal canal. The general aspect and condition of the patient seemed healthy.

The diseased organ was removed on the 15th of October, and the patient left the hospital, so far recovered, on the 10th of the present month.

A section of the testicle, after amputation, showed a congeries of innumerable cysts, varying in size from that of a small millet seed to that of a large hazel nut. Many of them were filled with a yellow serous fluid, and here and there were masses of cartilaginous material, deposited n nodules, and also bony matter. There were, besides, much of inflammatory matter and *debris* of waste cells, but the microscope did not indicate the presence of cancer cells, nor were there any spermatozoa. The diseased mass weighed five pounds, and was 22 inches in circumference.—*December* 13, 1862.

Ulcer of the Stomach.—Dr. Foot observed that he was indebted to the kindness of Dr. Stokes for the opportunity of exhibiting a specimen of ulcer of the stomach, which, after perforating all the coats of that viscus, eat deeply into the pancreas, and, finally, eroded a large artery of that gland, which led to a fatal hemorrhage. The patient was a woman advanced in life. She had, for a period of a month before her death, been subject to vomiting, and rejected the contents of the stomach every evening; but three days before her death she threw up a quantity of blood. She was at once carried to the Meath Hospital in a state of syncope. This occurred on Friday. On Saturday evening she had another attack of hematemesis; and at half-past one o'clock on Sunday morning blood began to pass from the intestines.

Syncope came on, from which she never recovered; she died at nine o'clock Sunday morning. On examining the body the greater part of the small intestines were found filled with blood. In the stomach were two pounds six ounces of coagula, some of which were moulded to the form of the viscus. The pancreas was closely adherent to the back part of the stomach. A well-defined ulcer, of an oval form, and deeply excavated, was found on the posterior wall of the stomach, towards the lesser curvature, and nearer to the œsophageal than to the pyloric end. The mucous membrane of the stomach itself was the only part of the body which seemed to have retained any of its natural allowance of blood; all

other parts being equally blanched and bloodless; the orifice of the eroded artery projected from the floor of the ulcer.—*December* 20, 1862.

Fracture of the Acetabulum.—Professor R. W. Smith exhibited two drawings, showing the appearance of the limb in a case of displacement of the head of the femur, resulting from fracture of the brim of the accetabulum. The patient, a man of about 40 years of age, fell from a plank, whilst carrying a weight of twenty stone, and received an injury of the hip-joint, November 22nd, 1860; he was at once brought to the Workhouse Infirmary of Enniscorthy, and placed under the care of Dr. O'Rourke, the medical officer of the establishment. The limb presented the appearances of a luxation of the head of the femur upon the dorsum of the ilium; the displacement was reduced without difficulty, but shortly afterwards recurred; it was again reduced; and from the facility with which the head of the bone passed in and out, Dr. O'Rourke concluded there was a fracture of the brim of the acetabulum. At the expiration of eight weeks the patient left the Workhouse Infirmary, and at the end of three weeks more, was received into the County Infirmary, where (the head of the bone being again out of its place) the dislocation pulleys were employed; he remained in the infirmary for three weeks, and then placed himself, for two months, under the care of a bone-setter, who enclosed the joint in a leather case. In the following summer he came to Dublin, and was admitted into the Richmond Hospital.

The appearances which the limb presented, were those of luxation of the head of the femur, upwards and backwards; but a close examination showed that it was not an uncomplicated displacement. The limb was shortened to the extent of two inches, and the foot was inverted. There was a very distinct and prominent tumour on the dorsum of the ilium, of a globular form, which followed every motion imparted to the femur.

So far the case resembled one of simple luxation, but it was found that the patient had the power of everting the foot, abducting the limb, and flexing the thigh, to an extent, not observed in cases of uncomplicated luxation on the dorsum of the ilium; these circumstances, connected with the history of the case, indicated a fracture of the brim of the acetabulum, and consequent displacement of the head of the femur, a form of injury usually productive of permanent lameness.

So long a period had elapsed since the occurrence of the accident that no attempt was made to remedy the deformity of the limb resulting from it, and the patient, after some time, returned to Enniscorthy, and instituted legal proceedings against his medical attendant, for negligence and malpraxis, but a verdict was very properly returned for the defendant. Professor Smith alluded to another case of the same nature, which he had recorded in the 12th volume of the *Dublin Medical Journal*.—*December* 20, 1862.

Diphtheria.—Dr. Stokes observed.—I beg leave to lay before the Society the pharynx and respiratory organs of a patient who recently died in the Meath Hospital, and which strikingly exhibit the anatomical characters of diphtheria. The patient was, to all appearance, perfectly well a fortnight ago. He then began to complain of hoarseness; and after the lapse of a few days, he was seen by Dr. Robert White, who, upon examining the throat, found a thick exudation covering the tonsils and pharynx. Recognising at once the formidable nature of the disease, Dr. White sent him to the Meath Hospital. His state on admission was as follows:—There was considerable tumefaction of the neck, rather more on one side than on the other; it was not, however, œdematous, and had not the character of the swelling so often seen in cases of scarlatina, or when the circulation is obstructed by a thoracic tumour; it seemed to be a glandular swelling; there was congestion of the face and great debility; his voice was weak and hoarse. He was subject to paroxysms of dyspnea of an alarming nature, but there was never that extreme degree of stridor which calls for the interference of the surgeon; they did not continue long, and when he was roused a little and given some wine, the attack would pass off, and his breathing become quite natural; the decubitus was upon the right side. On the second day after his admission, Dr. White saw him, with me, and on that occasion I had a consultation with my colleagues, Surgeons Wharton and Porter. It was the opinion of all that the case was not one for surgical interference. The patient sunk gradually, and died in a semi-comatose condition. There had been no fetor at any period of the case.

Upon examination after death, we found the posterior half of the tongue, the tonsils, and the pharynx covered with a thick and very tenacious false membrane, of a dusky yellow colour, in one place almost black; it also invested both surfaces of the epiglottis, which was extremely rigid, and the laryngeal surface of which was thickly coated with the morbid deposition. The deposit extended through the cavity of the larynx, and down the trachea and right bronchus as far as its fourth or fifth subdivisions in the corresponding lung, the left bronchus being perfectly free from it. The mucous membrane, beneath the deposit, presented a purplish colour, and a rough, mamillated surface, looking as if portions of the false membrane had penetrated its tissue, but nowhere did it exhibit any traces of ulceration. The right lung was so heavy that portions of it sunk in water; when cut into and squeezed, a serous-looking fluid ran out in streams; it was scarcely crepitating, and in parts seemed to have been passing into solidification, but without presenting the characters of pulmonary inflammation The left lung was comparatively healthy, and the left bronchus, as I have already stated, perfectly free from false membranes. This appears to me to be the most interesting point connected with the history of this case; for I cannot

find in that great *repertoire* of the literature of diphtheria, compiled by the Sydenham Society, any instance in which the false membrane was confined to the right bronchus and bronchial tubes of the right lung. The fact, as exemplified in this case, may, as far as it goes, be taken as an additional illustration, if such were wanting, of the greater sympathy that exists between the right lung and the larynx, than between the latter and the left lung.

The case which I have brought under the notice of the society bears upon the question of the true nature of this formidable disease. Is it a disease of exudation? or is it the result of some cause acting from without? Dr. Nicholson has examined the morbid deposit found in specimens before us, and, so far as his observations go, they tend to confirm the opinion of Bretonneau, that the disease is somewhat analogous to those affections of the skin which are now supposed to possess a vegetable character. It is found by Virchow, that in cases of simple or inflammatory croup the exudation rests, as it were, on the surface of the membrane, but that, in the disease under consideration, all through the epithelium, processes or roots of the false membrane are observed to pass through the mucous structure itself, and fix themselves in the connective tissue with the mucous membrane; and, further, that yellowish bodies resembling fungi in their microscopic character, are found under these circumstances. These appearances Dr. Nicholson conceives that he has completely verified in the specimen before us. I do not at all wish to advocate either the one or the other theoretical view of the disease, but certainly there are some circumstances connected with the case tending to the belief that it is a disease derived from some cryptogamic sporula, or contagion *ab extrâ*.

Its contagious nature, under certain circumstances, has now been well established by many admirable observations on the continent; the facts, too, dwelt on by Bretonneau, are very remarkable—that persons labouring under this disease, are subject to diphtheria of the skin; and also the circumstance that persons in the house with individuals suffering from diphtheria are liable, in the event of their skin sustaining any injury, to have the wound assume diphtheric characters. If a blister is applied to a person under such circumstances, the blistered surface is liable to take on features of diphtheria, and there are no anatomical distinctions between cutaneous and mucous diphtheria.

Another important point is, that no one who reads the memoirs of Bretonneau can doubt that the only treatment which is really useful in this disease is local treatment calculated to kill or destroy something; and that the only general treatment, calculated to be of service, is such as tends to support the patient's strength. This looks very like as if the disease was something derived *ab extrâ*, such as lichen, or a fungus. It is curious, that although this view of the German pathologists, as to the

vegetable origin of the disease, seems new, Bretonneau himself uses, in a botanical sense, the word "lichen," as expressive of the affection.

The last observation I shall make, in illustration of this theory, is, that in the present case there is but one lung and bronchus affected. If the disease were, like the secondary affections of fever, the result of a pre-existing morbid condition of the system, nothing would be more unlikely than that one bronchus should escape, while every bronchial tube on the other side should be affected. This circumstance is more in favour of the vegetable theory of this disease, than of the idea that it is merely the result of a constitutional malady.—*December* 20, 1862.

PROCEEDINGS OF THE DUBLIN OBSTETRICAL SOCIETY.[a]

TWENTY-FIFTH ANNUAL SESSION, 1862-63.

Dr. Beatty, President.

ADDRESS OF THE PRESIDENT.

We are assembled here to-night for the purpose of opening the 25th session of the Dublin Obstetrical Society, and I seize this, the first opportunity that presents, to offer my best thanks to the council of the society, in the first instance, for having selected and put my name forward, and to the members at large, who have done me the very distinguished honour of electing me president for the ensuing year.

The members of the society are aware that, at the close of the last session, an important change in its constitution was effected, and that a system that was considered to be too much of the oligarchical, was changed for one of a more democratic character. Under the late constitution the presidents, of whom there were four, and the vice-presidents, of whom there were fourteen, and the committee, of whom there were nine, were all self-elected, and were permanent—thus depriving the members at large of any voice in the selection of those who were to be the managers and directors of the proceedings of the society. It was thought by those who had the best interests of the society at heart, and who were best acquainted with the feelings of its members, and most alive to the great fact now so patent throughout the world, that communities will not be satisfied without a voice in the selection of their rulers; it was thought, I say very properly, that the time was come when the choice of the officers should be placed in the hands of the

[a] These reports are supplied by Dr. Geo. H. Kidd, Secretary to the Society.

members, and that they should be called upon, annually, to exercise that very important privilege and duty. Accordingly, a new code of bye-laws was drawn up by the committee, and submitted to the consideration of the society, and after several meetings, at which they were fully discussed, the code, now printed and distributed to the members, was adopted. We commence to-night under a new regime, and I have no doubt that very great benefits will result from the combined energies of all the members, who will henceforward feel that they have each, individually, an interest in upholding and forwarding the objects of this society, not only in reading and listening to papers at its meetings, but in the election of the officers who are to constitute its staff.

In future the council of the society will consist of one president, two vice-presidents, one treasurer, one secretary, and a committee of five, who will have no permanent tenure of their offices, but will be chosen annually, by ballot, at the first meeting of the session. To have been selected by my brethren to fill the chair of president, at the first election under the new system, is, I assure you, to me a source of the highest gratification and well-founded pride. Pride is a sentiment very often fostered, and built upon very unworthy foundations, and when it is so, it only subjects the individual in whom it is engendered to the contempt of his fellow men. But when, in the course of a professional life, after many years spent in very active public and private occupation, after filling professorships, and presidentships in other places, one finds the good opinion of his brethren undiminished, and their voices raised to place him in the highest position among them, he would be unworthy of the honour conferred upon him if he did not feel proud of such an exhibition of approval. The Dublin Obstetrical Society has now been in existence 24 years. It is the oldest institution of the kind in this empire. Dublin has the merit of having originated two most important and useful societies, connected with the medical profession, and set an example that has been followed by London and Edinburgh. This society is one, and the Pathological Society the other. In both these branches of scientific pursuit Dublin took the lead, and the great advantages arising from such institutions, having become manifest, our brethren in the metropolis of England and Scotland have wisely founded similar, and, I am happy to perceive, very flourishing societies. In speaking of the origin of this society, it should never be forgotten that we are indebted to the wisdom, energy, and foresight of a highly distinguished ornament of our profession for its suggestion and establishment. Dr. Evory Kennedy, then Master of the Lying-in Hospital, looking beyond the walls of the magnificent institution over which he was appointed to preside, soon perceived that, in the great advances then being made in all branches of science and human knowledge, there was a want of a centre, of a place of union, where, by a combination of the intellect and energies of the many

ndividuals engaged in the same pursuit, an impetus would be given to the acquirement and diffusion of information connected with our particular study. He instituted and watched over this society in its infancy, and its importance and value are best attested by the published reports of the able communications furnished at its meetings, and the way in which his example was followed in other cities. Dr. Evory Kennedy's name must therefore be ever associated with, and gratefully remembered by, the Dublin Obstetrical Society.

In November, 1844, now just eighteen years ago, I had the honour to deliver an address to this society, at the opening of its 7th session. When I look back through the years that have passed between that period and the present, I find much to excite feelings of very opposite character. Time, inexorable time, has plied his busy and fatal scythe among us, and has cut down a larger number of great and distinguished men than usually fall so close together. Crampton, Cusack, Marsh, Graves, Harrison, Williams, Rynd, Porter. All these have fallen since I last had the honour of addressing this society, and all of them have been at times assistants at our meetings. Some of them my seniors, some of them my fellow students, some of them my colleagues, all of them my friends. Deep regret and sorrow must arise in the breast of all who had the great happiness and privilege of knowing and living with such pillars of the fame of Irish medicine and surgery, whenever their honoured names are mentioned. There is one name I have omitted from the list, which I know must have suggested itself to every member of the society, and I have reserved it for special mention. You all expected the honoured name of Montgomery to appear among the list of extinguished lights. He was more particularly associated with us than the others were, and in the particular branch of medicine with which this society is concerned his loss is more seriously felt. The world at large, who only knew him as the brilliant and eloquent lecturer, the successful practitioner, and the thoughtful accomplished writer, have good reason to respect the memory of one who was a distinguished ornament to our profession, and to be proud of him who has left a monument, erected by himself during his lifetime, more lasting than bronze—his great work on the signs of pregnancy.

The members of this society, in which he held the office of one of its presidents, may recall with affectionate regard his eloquent and classical address, delivered in the year 1843, and his many valuable contributions at our meetings. But it is only those, in whose ranks I am happy to number myself, who knew him in early life, when the struggle for fame and position was before him, who can duly estimate his industry, his never-tiring assiduity, and the years of labour he bestowed on the making of his celebrated museum—for with his own hands he made it; many an hour I have spent with him in the workshop. The highest rewards in profes-

sional life naturally flowed in upon him, and he fell in the galaxy of grea men whose names I had previously mentioned.

But, gentlemen of the Obstetrical Society, if we have much to regret, we have, on the other hand, much to rejoice over and be proud of. During these last years great advances have been made in our particular branch of the profession; and I think it is not going too far to assert that, notwithstanding the brilliant improvements in surgery, and the solid and wise modifications in medicine, the changes in our art have preserved more lives, and relieved more human suffering and misery. The pains of the human female in giving birth to her offspring are proverbial, and are, in many cases, the greatest that nature can endure. Formerly we stood by, watching the agony, looking with deep anxiety for its termination, but powerless to alleviate, or diminish its intensity. Thousands of years have rolled away since the primeval curse, and countless millions of women have gone through the great trial of child-birth, without any means being devised to mitigate their woe, until, within a few years, it was discovered in America that the inhalation of ether was capable of removing the consciousness of pain during labour. The use of this agent having been attended with some inconveniences, it occurred to the acute and philosophic mind of Professor Simpson that there was probably some other of the class of ethers that would equally act as an anesthetic, without producing these unpleasant effects.

He accordingly instituted a long course of experiments, with a variety of vapourizing fluids, and concluded by discovering that chloroform was the one most to be relied upon to produce anesthesia with safety. This agent was at once approved of by the profession, and came into general use to assuage pain in all surgical operations, as well as in midwifery. It is a remarkable fact that, although serious and fatal accidents have arisen during its employment in surgery, there is no instance (as far as I know) recorded of its injurious or deadly consequence when administered during labour. Any one who has witnessed the sufferings of a young creature during the many, many, hours of a first confinement, who has heard the frantic, half maniacal shriek, and seen the fearful contortions of a frame agonized beyond the power of man to comprehend, will rejoice that he has, in chloroform, a means of alleviating such misery, that will soothe the excited, frenzied mind, calm the bodily suffering, and produce such a happy feeling in the previously distracted patient, that she often gives vent to the exclamation "oh! this is heaven." Any one who has attempted to perform the operation of turning in a tightly contracted uterus, with a patient utterly unable to control her plunging or her screams of agony, and then, having got the woman under the influence of chloroform, comes again to try what can be done, will be able to appreciate the enormous value of this agent, by the facility with which he is able to go through all the steps of the operation. His hand

passes easily, the child is turned without opposition, and the patient is delivered without pain. To have in our hands such an agent, that was unknown to our predecessors, and by which such marvels are worked, is, I repeat, a good ground for our rejoicing in the present state of our knowledge and power.

Another source of rejoicing is the present state of public opinion and practice respecting the midwifery forceps. When I compare the existing practice, with what it was fifty years ago in this country, it is to me particularly a subject of congratulation and pride. At the time I allude to, for some time before it, and for many years after, including a period of fully forty years, this instrument was banished from practice through the whole of this country. The feeling was so strong against its employment, and the leaders of the outcry were so powerful, that no one dared to question the authority by which it was condemned. The perforator and crochet were the only means in use, craniotomy was the only operation. This indiscriminate adoption of the mutilating instruments was the wonder of strangers, and was the opprobrium of the Dublin school. The submission to the authority alluded to was so complete, that I am almost perfectly correct in saying "no one dared" to question it. But there was one faithful priest who had kept the sacred fire burning in spite of all attempts at its extinction and, towards the close of a most laborious professional life, had the courage to proclaim the merits of the forceps, and the success that had attended their use in his hands. This was done at a time when even to mention the name of the instrument was considered a heresy, and nothing short of excommunication could be expected by him who was rash enough to recommend its use. That individual was the late Dr. Beatty, my father; his paper was read at the association of the College of Physicians, and published in the first volume of the new series of their Transactions. That first opened the eyes of Irish practitioners, who had for so long groped along in the darkness of error; but it was not to be expected that any very rapid change would take place. To convert an entire nation, long schooled in a particular doctrine, is a work of time; but the good seed was sown—105 cases in one man's private practice was proclaimed to the world, and the reformation was begun. The author of that paper died in 1831; and, in 1842, I read before this society a paper entitled "Cases Illustrative of the Use of the Forceps." There are some of the existing members who were present upon that occasion, and they will, I doubt not, recollect the feelings with which my observations were received by the then seniors of the society. One fact will suffice to show the prevalent notions respecting the forceps at that day. The chair of the society was occupied on that evening by one of the most esteemed and respected members of the profession—a gentleman who had been master of the Lying-in Hospital, and for forty years had enjoyed one of the largest

practices in this city, and on that occasion he stated, that he had *once tried* the forceps and failed. It was expected that the discussion on my paper would have been of an animated character, but the hour being late the discussion was postponed to the next meeting. I came to that, prepared to have my views canvassed, and my doctrine controverted, but there was no one to enter the lists—the opponents did not appear. Judgment was suffered to go by default. From that time a steady progress in the correct appreciation of the value of this instrument took place, additional testimony was borne by succeeding writers, amongst whom Dr. Churchill was the earliest and most powerful. The instrument assumed its legitimate position in the practice of all well-educated members of the profession, and we have lately had the gratification of hearing in this society, and afterwards of seeing in the *Dublin Medical Quarterly Journal*, a most valuable paper, by a late assistant of the Lying-in Hospital, Dr. Sinclair, "On the Timely Use of the Forceps." The rate at which the current now runs in favour of this instrument may be judged of by referring to two papers published in the first volume of the *Transactions of the Obstetrical Society of London*, for the year 1860. I allude to that by Mr. Harper, "On the More Frequent Use of the Forceps as a Means of Lessening both Maternal and Fetal Mortality," and also that by Dr. Tyler Smith, "On the Abolition of Craniotomy from Obstetric Practice in all Cases when the Fetus is Living and Viable."

While speaking of the forceps, I trust the society will excuse an allusion to a subject that nearly concerns myself. In the last volume (the third) of the *Transactions of the Obstetrical Society of London*, there is a paper by Dr. Graily Hewitt, entitled "On Unusual Elongation of the Fetal Head, as a Cause of Difficulty in the Application of the Ordinary Obstetric Forceps, with Description of a Modified Form of Instrument to be Used in such Cases." This is an admirable paper, and is accompanied with an engraving of the instrument—and a first-rate instrument it is, but is not new. No one has a better right to know the value of that instrument than I have, for it corresponds in every particular, to the most minute fraction, with my forceps, described and represented exactly twenty years ago, in the twenty-first volume of the *Dublin Medical Journal* for 1842. The measurements given by Dr. Graily Hewitt, correspond so exactly with mine that one would be disposed, at first, to think him guilty of piracy, but I entirely acquit Dr. Hewitt of any such intention. It is very likely he never read my paper or saw my engraving. If Professor Murphy had been present when Dr. G. Hewitt read his paper, and showed his instrument, he could have informed him of my priority, for he was present when my paper was read here, and has one of my instruments, and has written[a] strongly in its favour. I may be excused, I hope, for further stating, that I had, lately, the gratification to hear from Dr. M'Clintock,

[a] The last edition of his work.

the late master of the Lying-in Hospital, that the pair of forceps which I had the pleasure of presenting to him, when he was elected to the Hospital, was the only one he had used during the seven years of his mastership. The restoration of the forceps to its proper position has only been effected during the last twenty years, and its progress was slow until within the last ten years. It is a subject of much rejoicing to me to have lived to see such a revolution begun and brought to so happy a conclusion, and I congratulate the profession in this country on the power they now possess, and I particularly congratulate all unborn infants of the present day on the fact that the innocuous forceps, and not the deadly perforator, will be used to assist their entrance into this world, if necessary.

A form of disease in the human female, formerly utterly hopeless, has in these later days been brought within the reach of art, and a large number of successful operations attest the vast importance and value of the inventions. I allude to the wide and too frequently exhibited class of diseases of the ovary. Some forms of disease of this organ do not necessarily or rapidly terminate the life of the sufferer. Others again (the dropsies) soon give rise to such symptoms as to require the operation of tapping, which is after all but a palliative, and often is itself the immediate cause of death. In these cases where tapping is resorted to the operation has to be repeated sooner or later, and the average duration of life does not extend beyond two or three years. Some cases will be found recorded in which life has been prolonged for many years, and numerous tappings have been had recourse to, but these are only the exceptions. The radical cure of ovarian disease was the great desideratum, and I think we have arrived at a period in the history of medicine when we can announce that such has been found. It is not pretended that any proceeding has been discovered by means of which every sufferer from ovarian disease can be relieved from her malady, but from the accounts we now have before us, and the statistics deducible from them, we are justified in stating that the average of recoveries after the two kinds of operation that are now received, and practised, is fully as great as that after most of the great surgical operations, undertaken in cases which, without such interference, must terminate in death. That is the true test whereby to try the value of any operative proceeding. I have alluded to two operations now in general use—one of them suited to only one form of ovarian disease, the other to all. In the first, or what is termed ovarian dropsy, the disease consisting in an accumulation of fluid in one of the cells of the ovary, it has been found that if, after tapping and emptying the sac, a stimulating fluid be injected into it, inflammation of the sac is set up and the sac is obliterated, thus preventing any future accumulation of fluid in that quarter, and curing the disease. This operation has been very successful, and the number of

cases daily recorded fully justifies its adoption in all suitable cases. I have employed it myself with the most complete success, and I think it ought to be tried in all suitable cases previous to the adoption of the other and more serious operation—the complete extirpation of the ovary. This is one of the greatest achievements of the present day; its proposal was at first received with amazement and horror, and the first cases recorded were looked upon as the acme of rashness. From the original small incision of Jefferson, only an inch and a-half or two inches in length, the advance was rapid and startling, when M'Dowall of Kentucky, in America, Lizars of Edinburgh, and Clay of Manchester, laid open the abdomen with an incision eighteen or twenty inches long, from the ensiform cartilage to the pubes, exposed the ovarian tumour, separated its pedicle and adhesions, secured its vessels, and removed the whole mass entire. In recording his first case, operated on in June, 1842, Mr. Clay makes the following remarks:—"In many cases, as I shall afterwards prove, the mode pursued by Mr. Jefferson (the small incision) is not only impracticable but really absurd. I find connected with the Jefferson mode, eight cases, of which five were fatal. With that of Mr. Lizars four by himself, three by Dr. M'Dowall of Kentucky, one by Dr. Smith of Connecticut, one by M. L'Aumonier in France, and one by myself, making ten, of which only one was fatal, and that with great propriety might have been attributed to other causes." This was a most encouraging beginning. I will not weary you by giving a history of the operation from that to the present time, but I will just mention the results as published by some of those who have had most experience in this operation. Dr. Tyler Smith has lately recorded eight cases in which all were succesful but one. Mr. Spencer Wells has operated fifty times—of these thirty-three recovered, and seventeen died. Mr. Baker Brown gives the report of sixteen cases in which there were only two deaths; and Dr. Clay gives a retrospect of his operations from 1842 to the present time. The whole number of cases operated on by him was one hundred and four, of which seventy-two recovered, and thirty-two died. Dr. Clay thinks we cannot reasonably hope to extend our success beyond seventy per cent. When we compare these returns with the average recoveries after the great operations in surgery, we see good grounds to rejoice over methods, now within our reach, whereby a most formidable, and hitherto incurable, disease may be fairly encountered.

There are conditions of life, states of existence in which, although life itself is not threatened, nor any great bodily suffering endured, death would be almost preferable. To linger out a number of wretched years loathsome to herself, and intolerable to her nearest relations and friends, shunned by all acquaintances, and unable to mix in the every-day occupations of the world, has been the fate of many a wretched woman. In the great majority of these lamentable cases the condition of the woman

has arisen from mismanagement during labour, and hence the greatest number of them are found amongst the poor, who, in remote districts, have been obliged to depend on the more than doubtful care of the country midwife. Difficulty arises, delay takes place, time, valuable time, is lost in all sorts of foolish devices to expedite delivery. Dangerous pressure is going on all the while, destructive inflammation is established, and the mischief is done, no matter how the delivery is afterwards accomplished, whether by the unaided and persevering efforts of nature, or by the assistance of a medical man, called in too late to prevent the destruction of important parts, by the mortification that must ensue, and which effects are too often charged upon him, when the sad condition of the patient becomes manifest. Sloughing of the bladder takes place, an aperture, varying in size from that of a pin-hole to one of two inches square, in all directions is established, and all control over the organ is for ever lost. These are cases in which the "timely use of the forceps," as Dr. Sinclair so well terms it, would have prevented all the evil results. The unhappy victims of such a fatality have been, until a very recent period, condemned to a hopeless future. Various attempts of different kinds were made, from time to time, to relieve this terrible state, but none were of any avail. Failure after failure damped the energy and sickened the heart of every one who sought a remedy. The glory of triumphing over this hitherto unconquerable malady was reserved for our own time. Our American brethren took the lead in this great discovery. Dr. Marion Sims of New York, followed by Dr. Bozman, contrived, and practised successfully, an operation with sutures of silver wire, by means of which these apertures were closed and the patients were restored to comfort. An eminent Irish surgeon almost simultaneously succeeded in accomplishing the same feat, by a somewhat different method. Mr. Maurice Collis invented a mode of proceeding, by which he had perfectly succeeded in curing several of these cases, before the American operation was well known among us. All this had occurred within a few years, and now the operations for the cure of these fistulæ, as they are improperly termed, are performed with confidence and success in all parts of the empire. Cases, instead of being avoided and discharged from hospitals, are sought after, and encouraged to enter the wards, and, after operation, are sent home to their families, sound in body, happy in mind, and fit and able to take part in all former duties and pursuits. Having been myself a successful operator in such cases, I can bear testimony to the gratification produced in the mind by the satisfactory results that ensue. There is no operation that requires so much time and patience; from one to three hours I have spent in its performance. There is no opportunity of showing great dexterity, as in rapid amputations, or in lithotomy .By-standers get weary and depart, assistants are often worn out and wish it was over, but the operator cannot hurry; every step must be taken with

caution and care, and no matter what length of time it takes, he must take for his motto "patience." But, in success he has his reward; to feel that he has rescued a fellow being from the loathsome misery of such an affliction is worth going through any amount of labour and fatigue. This great triumph of art is another cause of rejoicing; and we, of the present day, have good reason to be thankful, that we have lived in the time when it has been contrived.

I have thus endeavoured rapidly to glance at some of the improvements which have been added to our store, during a comparatively short period, and I think I am safe in saying, that in no other branch of medicine have so many and so great additions been effected in the same time. Before I conclude I would take the liberty of suggesting to the members of this society, that an increased responsibility is now thrown upon them, and that we must take care that the reputation so long enjoyed by the profession in this city shall not suffer through any inertness of ours. We should look across the Channel and see what our brethren in the great metropolis have accomplished in the three years of their existence. The three volumes which I hold in my hand testify to the great energy with which the London Obstetrical Society has been worked. We should recollect that our mature years are expected to hold place with, if not to excel, our younger fellow-labourer. I have no hesitation in saying, that I think the papers in these three volumes of transactions reflect the highest credit on that society. I have every hope that the ensuing session of this society will be one of peculiar value, and nothing that I can do to forward its interests shall be wanting. We have a high vocation, and much is demanded from us; but above all things, while working with earnestness, exulting in our ingenuity, and taking pride in our instruments and handy-work, let us never forget that we ourselves are but instruments in the hands of the Great Physician, the Great Mechanician, without whose help and blessing all human efforts are unprofitable and vain.—*November* 29, 1862.

Large Vesical Calculus Expelled by the Natural Efforts, by a Woman in the Seventh Month of Pregnancy.

Dr. J. A. Byrne exhibited a calculus expelled by a woman seven months pregnant.

The history of this case was this:—It was the woman's second pregnancy; since the commencement of it she had suffered from symptoms of vesical irritation, which had gradually increased in intensity, and caused a good deal of pain, frequent desire to make water, &c., &c.

When she was approaching the end of the sixth month these symptoms had become very much aggravated; and one morning, after arising from bed, she found that on attempting to make water, not a drop would pass.

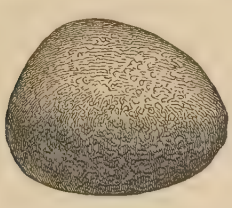
This, of course, astonished her a good deal; and frequently during the day and night she repeated the attempt, but with the same result. She felt all the symptoms indicating retention, but, notwithstanding her repeated efforts, she could not succeed. She passed a most restless night, as may well be imagined, and resolved to seek advice on the following morning.

Before, however, doing so, she determined to make one more effort, and, in sitting down upon the chamber, and using some force, she heard something falling into it, and a great gush of urine followed; on looking into the vessel she perceived, to her astonishment, this stone, which she afterwards brought to me, and thus I have the opportunity of showing it to the members of the society.

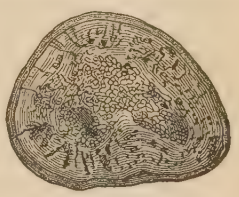
After this she experienced no further annoyance, all the symptoms of urinary irritation subsided, and she was delivered, at the full time, of a healthy male child.

From the time she passed the calculus she had not the least *incontinence of urine.*

The retention of urine continued for 24 hours exactly, and no longer, otherwise some temporary incontinence might have followed on account of the large size of the calculus, which is more than one inch in length, is an inch in breadth, and three-quarters of an inch in thickness. It weighs 119·33 grains, and its chemical composition is tri-basic phosphate; its outer surface is smooth and easily broken up, but its nucleus, although of the same composition as the outer part, is more dense and harder. On looking at the calculus, of which a drawing is annexed, as also one of its section, we cannot fail to observe its well-marked wedge



Calculus.



Section.

shape; it is obovate, and it is highly probable that it entered the meatus by its narrow extremity, and that, by degrees, it passed along, dilating the urethra, until finally it was expelled in the sudden manner that has been mentioned. Now it was fortunate for the woman that such was the case, as it would have been difficult for the large extremity to have entered the meatus; once lodged, however, in this short canal, it began to dilate it, and that quickly. It is interesting in a surgical, point of view, that during the period of retention no surgeon was called in, because he could not have failed to feel the calculus with the catheter, and of course would have taken

means to remove it, and thus this case, so strongly illustrative of the dilatability of the female urethra, would have been lost to science.

It is worth mentioning, that during her first pregnancy she did not suffer from any stone symptoms whatever, and the interval between her pregnancies was only 12 months, so that either the calculus must not have given any annoyance, or it must have been developed during the early seven months of her second pregnancy.

Dr. Byrne drew the attention of the society to the case mentioned by Cazeau, in his *Art des Accouchments*, in which he mistook a calculus for the fetal head, descending upon the point of the finger, in trying for *ballottement*, and was very nearly pronouncing a woman pregnant; however, on examining further, he detected the stone floating loosely in the bladder. And also to the case detailed by Dr. M'Clintock, which occurred in the Lying-in Hospital whilst he, Dr. Byrne, was assistant; in this case (the woman's first pregnancy) the symptoms of urinary distress were extremely severe; the woman passed blood and pus, mixed with urine, and an operation to remove the stone became necessary; the stone, however, in Dr. M'Clintock's case, was rough and lodged in a sac. The removal of it was followed by recovery.

The two cases, he thought very interesting as complications of pregnancy.—*December* 13, 1862.

Dr. J. R. Kirkpatrick exhibited a *Congenital Nevus*, situated on the anterior part of the right thigh of a child born in the Rotundo Lying-in Hospital last November. The nevus was the size of a large hen egg, and extended from just below Poupart's ligament to the knee. Externally it was of a dusky red colour, and had many superficial veins running from the circumference towards the centre, where the integument appeared very thin, and disposed to slough. Internally the nevus was composed of innumerable meshes of veins, some of which were dilated into large sinuses. The sartorious muscle passed through the under surface of the tumour, and was surrounded by several loops of veins. The child died on the fourth day after birth. In every other respect it was a well formed and fine child. No thrill or pulsation could be felt in the tumour during life, except on the inner side, apparently from a superficial branch of artery.—*December* 13, 1862.

Dr. M'Clintock read the following paper on *Dropsy of the Ovum.*

It must be confessed we know very little of the pathology of this complaint, or of the special conditions which give rise to it. In very many of the cases which have fallen under my observation I have instituted careful examinations on these heads; yet, in but few examples was there any notable appearance of disease of the amniotic membrane. In these

exceptional cases the amnion was partially opaque and thickened, but nothing more. That the disease does not depend on a dropsical diathesis of the woman herself is shown by the fact that these women are often free from dropsical effusions in any other part of the system; and also, that very many patients are affected with general dropsy at the time of delivery, in whom, nevertheless, there is no marked redundancy of the amniotic fluid. The former of these statements is strikingly illustrated by the following case:—

Case I.—A lady, aged 33, in her tenth pregnancy, enjoyed very good health up to the beginning of the seventh month of utero-gestation. She then began to observe that the size of the abdomen was augmenting in a very unusually rapid manner. This went on for about a fortnight, when the belly had attained so enormous a magnitude as to be productive of extreme pain and distress. At this stage it was that I first saw her. She was a tall spare woman, and entirely free from anasarca. The abdomen was immensely swelled, tense, and obscurely fluctuating. She was uneasy in every attitude or position, but most so when lying down or recumbent; though she did not complain of dyspnea. The pain of the abdomen had deprived her of all rest for the three previous nights. The urine was scanty and turbid; the tongue clean.

I prescribed for her some diuretic mixture, and an anodyne draught. The next day I found her no better. She had passed a most wretched, sleepless night, propped up in an arm chair. On making an internal examination, the edges of the os uteri were ascertained to be very thin, and the orifice expanded to the size of a halfpenny; the membranes protruding and extremely tense: no presentation of the fetus distinguishable. The membranes were easily torn with the nail, whereupon a volume of water instantly gushed out, and, almost at the same time, the head of the fetus; its birth was retarded as much as possible; but the temporary obstruction—caused by the child—being removed the torrent began afresh, filling every available vessel, and deluging the bed and floor, within the brief space of a minute or two. The child was a female, and its funis pulsating; but it made only a few feeble efforts at respiration. The uterus contracted tolerably well, throwing off the placenta in twenty-five minutes, without hemorrhage. Her convalescence proceeded most satisfactorily, and the only deviation from the ordinary course of things was the size of the uterus, which remained inordinately large for a considerable time.

This lady had premature confinements on two former occasions, under circumstances very similar to the present, with this difference, that the quantity of water was not quite so great. These two children also were females.

Dr. W. D. Moore, of South Anne-street, was good enough to examine a specimen of the liquor amnii, four hours after delivery. Its specific

gravity was 1004; it had a slight alkaline reaction, and contained a trace of albumen.

The case above narrated shows that there is some liability to a return of the disease in a future pregnancy, although such an occurrence is far from common. When *hydrops amnii* affects a woman pregnant of twins, we usually find that the amnion of one child only is engaged; indeed, it very seldom happens that both ova are dropsical. Curious to say, it is the amnion of the second born twin in which the liquor is redundant. Thus, of *eleven* twin cases in which the disease was present, it was confined to one amnion in all of them; and in every instance, with two exceptions, it was the amnion of the second twin.

The dropsical effusion being confined to one ovum is a fact that plainly implies its cause to be of a purely local kind. That the fetus of the dropsical bag of membranes should be the last expelled most probably is due to its greater mobility, and to the greater ease with which it can be displaced towards the fundus of the uterus, by its more circumscribed companion.

Case II.—A large, florid-faced woman, aged 38, in her twelfth pregnancy, was admitted into the hospital, December, 1854, with spurious pains. The abdomen was of immense size, indistinctly fluctuating; the legs were œdematous, and she suffered from cough and dyspnea. The os uteri was patulous, but the membranes were not pressing into it; fetal heart inaudible. She was in the ninth month, and had noticed that, within the last fortnight, she had undergone a great augmentation of bulk.

The pains having gone off, she left the hospital; but returned again in a few days. She was confined, *en route* to the hospital, of a putrid child; and on rupturing the membranes of the second twin, after her admission, about seven quarts of water were discharged. This child appeared rather more decomposed than its fellow.

All her other children—ten in number—were born alive. On the present occasion the children were females, and the umbilical vessels of the first twin separated from one another at some distance from the placenta, and ran into the edge of the latter, widely apart, constituting the *insertio velamentosa* of the old authors.

Case III.—A Scotchwoman, aged 24, seven months advanced in her fourth pregnancy; former children born alive at term. When admitted she was not in labour, but the tongue was furred, the pulse frequent, and she complained much of pains in the loins; urine natural in quantity and appearance. The abdomen, which was 43 inches in circumference, presented well-marked fluctuation. No œdema of the labia, legs, or feet. Up to three weeks previously her size had been natural for the period of pregnancy, but since then she had rapidly enlarged. The history of this case, the absence of dropsical effusion elsewhere, the existence of

pregnancy, and the shape of the belly, all concurred to point it out as one of hydrops amnii. But, on the other hand, there was some enlargement of superficial veins of the abdomen; and, on vaginal examination, it was ascertained that the cervix was not yet obliterated, and that the presenting fetal head *was pressed so strongly against the lower part of uterus as to be scarcely moveable.* This circumstance was the chief difficulty in the way of making the diagnosis of dropsy of the amnion. She took her labour in a few days afterwards, and quickly expelled a putrid female fetus. The existence of a second was ascertained, and its membranes were ruptured, when between two and three gallons of liquor amnii came away. Here, then, we had a satisfactory explanation of the impossibility of practising *ballottement* in the first instance. The second child, a girl, presented with the feet, and was extracted alive, but in a weakly state, so that it only lived a few hours. This woman got a sharp attack of metritis on the second day, which was successfully treated by leeching &c., and she went home quite well.

The great ease with which *ballottement* can be performed is generally laid down as a diagnostic mark of dropsy of the ovum. The case just related plainly teaches us, however, that, in certain cases of the disease, we are to expect the presenting part of the fetus to be unusually fixed at the os uteri.

To make the diagnosis of twins, in the absence of any auscultatory evidence, is very rarely possible; but to do this, and at the same time to diagnose a dropsical condition of the second ovum, would seem, *prima facie*, to be quite beyond our reach; and I am not aware that such a diagnosis has ever been made in midwifery. However, with the data before us that I have just stated, it would appear, in a given case, to be a legitimate deduction.

This disease shows itself more commonly in subsequent than in first pregnancies. Of *thirty-three* of my cases, where this point was specially noted, only *five* were first labours; *eight* were second labours; *one*, a twelfth; and the rest intermediate. Occasionally it takes place very slowly and imperceptibly; but in by far the larger proportion of cases it was, comparatively speaking, rapid in its development; and the knowledge of this circumstance in the history of the complaint will oftentimes aid us in arriving at a diagnosis. Sometimes it was attended by symptoms of a rather acute kind, such as thirst, fever, diminished secretion of urine, and much abdominal pain and uneasiness. This last would seem referable more to the distension of the uterus than to any other cause.

Case IV.—A lady of very lymphatic temperament, aged 28, when in the latter end of her third pregnancy, came under my care, complaining of orthopnea, irritable stomach and bowels, thirst, sleeplessness, and a constant dull pain in the right hypochondrium. The uterine tumour was unusually large for the period of pregnancy (the end of eighth month),

but there was no œdema of the labia, legs, or feet. The pulse was generally small and frequent. I may remark, that for many months her health had been very delicate. Treatment succeeded in relieving some of the more urgent symptoms, but the abdomen continued to enlarge, and with this increase the dyspnea became more distressing, so that she spent all her time in a sitting posture.

A few days afterwards she fell in labour; and on my arrival at her bedside, finding the os uteri somewhat open, and the head or breech presenting (the mobility of the part prevented my ascertaining which), I broke the membranes, whereupon the breech descended, and there escaped about four quarts of liquor amnii. From the time of delivery all the dyspnea and abdominal pain entirely subsided, and for the first time for some weeks, she was able to enjoy the luxury of a refreshing natural sleep in the recumbent posture. She made a good recovery, though the frequency of the pulse for the first few days caused me considerable uneasiness.

Case V.—A woman, aged 29, in the seventh month of her sixth pregnancy, was admitted to one of the chronic wards of the Lying-in Hospital, December, 1857. About six weeks before, she began to experience crampy pain in the belly, which was larger than usual for the period of gestation. Shortly afterwards there came on a sanguineous discharge from the vagina, which recurred again and again, in variable quantity, and at times was quite watery or serous.

When the discharge was absent for some days the abdomen enlarged and the uterus became hard and painful. These symptoms went away when the discharge recurred, which it often did in gushes. It was at first supposed that this might be a case of hydatids, or cystic disease of the ovum, but auscultation showed this to be highly improbable, if not impossible, as the fetal heart was distinct and loud, and hydatids never, or almost never, coexist with a live fetus. The placental soufflet was also plainly audible on the right side of the uterus, extending up to near the fundus. The os uteri was thick, and abraded on its anterior lip. Pulse 110, and irritable. Cold enemata, astringent injections into vagina, and mild cauterization of the os uteri, were employed, but without any effect whatsoever on the discharges, which were at times very profuse, and consisted of a reddish water. Some days subsequently she took her labour, and, after four hours' illness, expelled a living female child, which presented with the breech. Immediately following the placenta was a mass of coagulated blood and fibrin. The placenta was healthy, but of a rather pale colour and infirm structure. Her pulse continued frequent for some days after delivery, but she, nevertheless, made an excellent recovery.

There can be little doubt that this case was one of dropsy of the amnion (or chorion), the fluid at times making its escape externally, and

always with marked relief to the abdominal pain. The hemorrhage arose from "accidental detachment of the placenta," produced, probably, by the alternate distension and relaxation of the uterus.

A dropsical state of the amnion is a very common morbid condition of abortive ova; and I cannot help thinking, with the late Professor Andrew Retzius, of Christiania (who drew my attention to this fact when looking over the Museum of the Lying-in Hospital), that it is a very frequent cause of the early death and expulsion of the embryo.

Of thirty-three cases of amniotic dropsy carefully noted by me, *one* ended in abortion at the fifth month, and *one* at the sixth month; *ten* resulted in the decidedly premature expulsion of the fetus; and in the remainder the child seemed to have, at all events, reached the ninth month, though in some of them it most probably had not completed it.

There seems good reason to believe that some of the cases of the disease called "hydrometra," or dropsy of the womb, were of the kind now under consideration—were, strictly speaking, a disease of the ovum, and not of the uterus; just as the so-called hydatids of the uterus is, in truth, a disease affecting only the involucra of the embryo. Many of the recorded cases of "hydrometra" admit of a much more rational explanation of their history and phenomena, and one more consistent with the physiology of the uterus, on this supposition than on any other pathological view.

This morbid excess of the liquor amnii, or, perhaps, the morbid action from which it results, seems to be very unfavourable to the well-being of the fetus, as *nine* of the children were dead born, *five* of which were in a putrid condition; and *ten* of the live-born children died within a few hours after birth. It occurred more frequently with female than with male children, in the proportion of *twenty-five* of the former to *eight* of the latter. The great difference between these numbers is very remarkable, and would almost suggest the probability of there being something more than a mere accidental association of this disease with children of the female sex.

The presenting part of the child was noted in *thirty-one* instances—and among these the head presented on *twenty* occasions, the pelvic extremity *nine* times, and the upper extremity *once*. We could not, from these numbers, safely infer that a redundancy of the liquor amnii favoured preternatural presentation, because there is another circumstance connected with these cases which would go far to account for it, namely, the great frequency among them of premature labour—of the *thirty-three* cases nearly one half having terminated in the premature expulsion of the ovum.

In the acute form of this disease it would perhaps be rather difficult to trace the symptoms to their real cause, unless the dropsical condition of the amnion was discoverable by physical examination. The enlarged uterus may be confounded with plural pregnancy, or with ascites. Where there is much distension of the uterus a feeling of fluctuation will be

communicated, with great distinctness, to the hand on percussing the abdomen, so that, in extreme cases, it would be impossible to arrive at a positive conclusion by this mode of examination. Internal examination will generally supply stronger evidence. The expanded state of the cervix, the extreme tension of the lower segment of uterus (or of the membranes, if the os be open), and the ease with which the child can be displaced, are all corroborative signs of the ovum being unusually distended with fluid. With reference to the last-mentioned sign, we must bear in mind what occurred in Case III., already related, which clearly shows that a dropsical ovum may coexist with immobility of a presenting fetus.

Some writers have stated that inaudibility of the fetal heart's sounds, and of the placental murmur, are results of amniotic dropsy. But this observation can only be received in a very qualified manner. No doubt, in a considerable proportion of these cases the child *is* dead, and its heart, therefore, inaudible; in many of them the patient is still far removed from the end of pregnancy, and this in itself creates a difficulty to hearing the fetal heart; but, in these and the remaining cases, we may generally succeed in detecting it, as I myself repeatedly have done, by persevering and diligent exploration.

In a few instances I have heard the placental murmur distinctly, and with all the characters belonging to it. In these cases I discovered it accidentally. Not having sought for it in any of the cases, I cannot say whether it is more or less easy of detection than under ordinary circumstances. The entire superficies of the uterus is certainly very much increased, but in the same proportion is the placental area increased; and I have little doubt it is the interference arising therefrom to the placental circulation which causes so many of these fetuses to be born in a dead or dying state. The first stage of labour is very commonly protracted in these cases from inertia, caused by the over-distension of the uterine fibres. Occasionally the cervix is completely effaced, and the mouth of the womb in a somewhat dilated state for days; but the uterus seems incapable of making any vigorous effort to expel its contents, or to rupture the membranes. Case I. was a good example of this, as was also the following:—

Case VI.—A woman, seven months pregnant of her eighth child, presented herself at the Dispensary of the Lying-in Hospital, complaining of pain in the belly, and of her unnatural size, which led her to think she must be "distended with wind or water, in addition to being with child." The uterine tumour was larger than it ordinarily is at the end of the ninth month, and this abnormal increase of bulk had taken place, she says, within the last fortnight. Her feet were slightly œdematous. Dropsy of the amnion was suspected, and this opinion was confirmed on examination. The lower segment of the uterus was full and tense, the

os uteri enlarged to the size of a shilling, and the membranes pressing strongly into it, and no part of the child could be felt presenting. She would not remain in the hospital, though urged to do so on the ground that her labour had set in. This, she said, was impossible; or, if so, she must have been in labour for fully five days; and it certainly seemed not altogether improbable that the uterus had been making slight, but ineffectual parturient efforts throughout this period. Some hours after leaving the hospital the membranes gave way, and several quarts of water were discharged. A premature child was expelled footling; but owing to delay between the birth of the trunk and of the head, through want of assistance, its life was lost. I got Mr. Connolly to make a drawing of the child, as it presented some remarkable appearances. There was a total absence of the thumb of the right hand, which was in a state of forced flexion. The umbilical aperture was very large, and through it protruded the spleen and small intestines. These, together with several ounces of clear amber-coloured fluid, were contained in a cyst formed by the membranes of the cord. An inch on the placental side of this cyst was another globular swelling of the cord, about the size of an orange, and likewise containing yellow fluid. The preparation of this fetus is in the Museum of the Lying-in Hospital.

A question of deepest interest connected with this disease of the ovum is its influence on maternal mortality. Now, of the 33 cases here reported, four ended in the death of the mother. One was from rupture of the uterus (the child, in this instance, was hydrocephalic); another from puerperal fever, at the time epidemic; and the other two deaths were from debility and prostration. Each of these latter women had been confined of twins, and were in a broken down state of health at the time of delivery. The above results will abundantly justify the observation, that a comparatively high rate of mortality may be expected amongst patients who are the subjects of dropsy of the ovum. One very influential reason for this may be found in the fact, that a large proportion of these women are in a more or less cachectic debilitated condition of body; in fact this state may be regarded as a strongly predisposing cause of the disease. Of the women who recovered, *three* had smart attacks of uterine inflammation after delivery. Altogether, then, it is plain that these patients, more than others, require to be closely watched during childbed.

I know of no treatment capable of arresting the secretion of the liquor amnii, or of causing its absorption where already secreted to an excessive amount. I have made trial of mercury, hydriodate of potash, diuretics, and other remedies, but could not perceive any good effect from their employment. Of course, when the distension of the uterus becomes enormous, puncture of the membranes must be performed, even though labour may yet seem distant, in the natural course of events. The

patient may suffer much pain and doiscmfort, apparently connected with the disease in question—as, for example, in Case IV.—but where as yet the distension of the uterus is not so great as to justify a measure which we know will bring on labour. If it were possible to draw off only a portion of the fluid contents of the amnion, perhaps labour would not immediately follow, and doubtless great relief would accrue to the patient, who could thus be carried forward in her pregnancy.

The history of Case V. furnishes us with a very good precedent for such a practice. Here nature seems to have tapped, as it were, the dropsical ovum from time to time, and let off a quantity of the superfluous fluid contained within it. How to explain the mode by which this was effected I am quite at a loss; as we have seen, however, the patient always experienced very great relief by these discharges; and it is more than probable that but for them her pregnancy would not have gone on so long as it did; its premature occurrence eventually, was, without doubt, the consequence of the severe hemorrhages.

The effect of this super-excess of liquor amnii upon the process of labour is generally, indeed I may say always, to retard it. The pains are constant, but not severe—not amounting to the degree of a regular labour pain; but yet they are most teazing to the patient. Very many hours, or even days, as we have seen, may be consumed in this way, before the os uteri will have reached the size of a penny. Under circumstances such as these, and where the accession of labour has evidently taken place, there may be no hesitation about letting off the water, with such precautions as the position of the fetus may call for.

In the heading of this article I have called the disease we have been considering "dropsy of the ovum," because it may fairly admit of question, in some cases, whether the excess of secretion be in the amnion or in the chorion. Every accoucheur occasionally meets with instances where there are "false waters," or liquor chorii; and hence I see no reason why this secretion may not occasionally be in excess as well as the liquor amnii. This would seem to have been the case in the following instance, which was communicated to me by my valued friend, Dr. Brunker, of Dundalk:—

Case VII.—The patient was in her ninth pregnancy; and, from her great size and evident abdominal fluctuation, she was supposed to have ascites coexisting with pregnancy. For some days previously to Dr. Brunker seeing her, the abdomen had so rapidly increased, and to such an extent, as to be productive of extreme bodily inconvenience and embarrassment to respiration. The patient had not been able to lie down for days. The belly was distended, apparently to the utmost degree; and on percussing it a most distinct fluctuation was felt. It was agreed between Dr. Brunker and Dr. Browne (the gentleman in attendance) to puncture the membranes, and so permit the evacuation, of the waters.

This was easily done, and an enormous quantity of fluid came away, with great relief to the patient. Labour supervened in 12 hours, when the woman gave birth to twins, and with each fetus "there was an unruptured bag of membranes." The children appeared to be of seven and a half months' development; they breathed feebly, and did not long survive. The patient made a very good recovery.

One case more I must be allowed to relate. There were other circumstances about it besides the amniotic dropsy which render its history interesting.

Case VIII.—E.D., aged 40 years, seventh pregnancy, was admitted to the Lying-in Hospital, 21st February, 1857. Soon after her admission the membranes ruptured, and upwards of *eight quarts* of bloody liquor amnii were discharged. The uterus immediately contracted pretty firmly on the child. Upon making a vaginal examination the greater part of the placenta was found protruding through the os uteri, and an arm of the child presenting. Some hemorrhage was going on. As the uterus was contracting strongly she was put under the influence of chloroform preparatory to the operation of turning. On introducing my hand for this purpose, I discovered that the os had contracted so much as to permit only two fingers to get through it: however, with these two I was fortunate enough to seize and bring down a leg. Much trouble was experienced in extracting the body of the child, which was a female, and in a decomposed state. On the second day she had a rigor, followed by rapid pulse, hot skin, and a dry brown tongue, but no uterine pain or tenderness. She was ordered large doses of James' powder, four ounces of brandy in the day, and poultices to the belly. On the third day the pulse was 100, and on the fifth day it had fallen to 88. She was quite well and able to go home on her ninth day.

I have prefaced this paper with the observation that we know, as yet, very little of the pathology of dropsy of the ovum. Of its particular cause, it must be confessed, we know absolutely nothing.

Still, the facts I have brought forward have a bearing upon the general pathology of this disease, and enlighten us upon some important points connected therewith.

1. Dropsy of the ovum certainly does *not* depend on any dropsical diathesis of the patient herself. This fact, I think, is clearly established.

2. There is no doubt, also, that it may occur quite independently of any dropsical condition of the fetus, such a concurrence being but very rarely met with; and hence being, we may presume, purely accidental.

3. It is not constantly associated with any appreciable morbid condition of the membranes or placenta; at least in some exceptional cases only did they present any deviation from their ordinary appearance.

4. The cause of the dropsy would seem to be a purely local one. This

is inferrible from the frequently observed fact that in plural pregnancies, where the disease was present, it was confined, in every instance, to one ovum only.

5. My experience of this disease does not lead me to think that it has any connexion with syphilis. In only one of all the cases on which these observations are founded was there any just ground for supposing the woman to be infected with syphilitic poison. In this exceptional case, father, mother, and child, all presented unequivocal venereal symptoms.

With regard to the *functions* of the amnion, we meet with three classes of facts which help to illustrate them:—

1. Cases of plural births occasionally are met with in which one of the fetuses having been blighted, the fluid contents of its enveloping amnion have disappeared. This I would attribute to its absorption by the membrane itself.

2. Again, we meet with cases in which the liquor amnii presents a very altered appearance—all its sensible characters being changed, and, instead of being transparent, thin, and inodorous, it is thick, turbid, and fetid. This is an instance of morbid or perverted secretion.

3. Lastly, we have seen that the quantity of the secretion may be enormously increased.

These facts all concur in strengthening the analogy between the amnion and serous membranes, by showing that it can, under certain circumstances, *increase*, *absorb*, or *alter* its proper secretion.

All this does not carry us far, however, towards explaining how the disease in question is produced. But I believe we must rest content with this limited amount of knowledge till physiologists make us better acquainted with the vital constitution of the amnion and other temporary organs concerned in the nutrition and development of the fetus.—*December* 13, 1862.

TRANSACTIONS OF THE COUNTY AND CITY OF CORK MEDICAL AND SURGICAL SOCIETY.[a]

SESSION 1862-63.

Dr. W. C. Townsend, President.

Heart Disease and Aortic Aneurism.—The President exhibited a pathological specimen, and read the following remarks:—

Thomas Galwey, aged 52, served 21 years in India, in 4th Dragoons;

[a] These Reports are supplied by Dr. T. W. Belcher, Secretary to the Society.

of very intemperate habits; admitted, for the first time, into the Workhouse Hospital about 11 years ago, under the care of Professor O'Connor. Since that time he has been frequently in hospital; and for the last 12 months constantly so. I saw him for the first time in January, 1857, when he gave the following history of his case:—Some years before he left India he received, from a horse, a kick in the chest, which rendered him insensible. He was at once taken to hospital, bled, leeched, and cupped. He remained in hospital several months, and was then discharged. He continued to serve as a soldier, but suffered more or less from palpitation, particularly when he made any exertion, or had—to use his own language—a bout of drink. After the return of the regiment to England, he was discharged, owing to his having absented himself without leave, on a drunken frolic. He presented a worn appearance, but stated that he did not suffer from cough, pain in the chest, difficulty in swallowing, or, in fact, any other inconvenience, except the palpitation. With that exception he enjoyed very fair health. When standing at his bedside a loud cooing murmur was distinctly audible. On placing my hand over the precordial region the vibration and increased impulse was most remarkable; percussion showed a great extent of precordial dulness. On examination I found a loud double murmur clearly audible over every part of the chest, but most intense about the middle and upper portion of the sternum; the pulse clearly indicated aortic regurgitation. After remaining in hospital for some time he left, and enlisted in the West Cork Artillery, where he acted as groom to the colonel, but was unable to do any duty. Since that he has spent his time in hospital, with the exception of about three months last autumn, when he was in charge of an orchard near the city. For the last nine months his health had been declining; he wasted very much, and suffered from occasional attacks of hurried breathing, which usually came on at night; still his spirits were good, and he enjoyed very fair health. On the morning of the 17th of September he was attacked with violent dyspnea, and died in about six hours.

On examination the body presented considerable emaciation. On opening the thorax the lower part of the left lung was found collapsed, and adherent to the lower and posterior part of pleura, with effusion into pleura (right lung not examined).

The pericardium healthy; no effusion into it. Heart very much enlarged; weighed (with arch of aorta and about three inches of trachea) two pounds nine ounces. The valves of right side of heart pretty healthy; the left ventricle very much enlarged, and the auriculo ventricular valves much thickened and uneven, from cartilaginous deposit. The aortic valves were very much thickened at their free margins, and turned down into the ventricle. There was a small true aneurism at the right side, immediately above the sinus of Valsalva.

At the first curve of the arch of the aorta, situated on its posterior wall, was a sacculated true aneurism, rather larger than a small nut; around its orifice and on its walls, situated beneath the lining membrane, were several calcareous deposits (after a few days maceration the lining membrane was clearly demonstrated). About a quarter of an inch further on, and more inferior, was a sacculated false aneurism, about the size of a lemon; its orifice was about the size of a shilling, and around the aperture the middle and inner coats were grown together, and had become much thickened. The sac contained a large quantity of decolourized fibrin, a great part of which had undergone degeneration, apparently of an atheromatous character. No rupture of the sac could be found, or communication with anything but the aorta.

Externally the sac was adherent to the trachea, at its bifurcation, and also for about half an inch to the true aneurism. In the descending cava, about a quarter of an inch from the vena innominata, was a red zone extending round the vein. The left vena innominata appeared smaller than natural.—*October* 8, 1862.

Notes on the Treatment of Continued Fevers, and other Acute Diseases.[a] By T. W. Belcher, M.A. & M.D., T.C.D., B.M. & M.A., Oxon.; Licentiate of the King and Queen's College of Physicians in Ireland; Physician Extraordinary to the Cork Fever Hospital, &c., &c.

Ἐν γε τοῖσιν οξεσι τῶν νουσημάτων τοσόνδε διοίσουσιν ἀλλήλων οἱ χειρωνάκται, ὡς τε ἃ ὁ ἕτερος προσφερέι ἡγεύμενος ἄριστα εἶναι, ταῦτα νομίζειν ἤδη τον ἕτερον κακὰ εἶναι

ΙΠΠΟΚΡΑΤΗΣ.

About two years ago I had the honour to read, before the Cork Medical and Surgical Society, a paper on the treatment of acute diseases, illustrated by several cases of pneumonia.[b]

I then clearly proved, I respectfully submit, that the cases referred to recovered without bleeding, tartar emetic, or mercury: and that they recovered in a much shorter time than is usual with persons treated on the older methods.

The plan—not by any means original—explained in my previous paper, consisted in using remedies to promote the free action of the skin, kidneys, and, to a lesser extent, that of the intestinal mucous membrane; with constant local stimulation, and upholding the powers of life by *food* capable of assimilation, such as beef tea and alcohol.

[a] This paper was first read before the Cork Medical and Surgical Society; and subsequently, in its present revised form, before the Regius Professor of Physic in the University of Dublin, as a thesis for the Doctorate in Medicine.

If any apology be necessary for presenting it to the profession, it must be simply this—the incalculable importance of the subject, and the duty of every physician to bring *facts* to bear on a controversy which is of vital importance to the community.

[b] See Dublin Journal of Medical Science, Vol. xxxi., p. 446, &c.

This proceeded on the very reasonable hypothesis that the disease, instead of being attended with undue exaltation of the vital forces, was, in fact, a disease of nutrition; that the vital powers were depressed, and required support to conduct the system through an ordeal which could not be cut short.

It, doubtless, appears plausible to assert that the want of appetite, weakness, and wasting, occurring in acute diseases, are natural processes intended to act as "a set off" against the presumed exaltation of the vital forces; that in fact life is at so high a pressure that food would burst the safety valve; and therefore the less of it the better.

A more natural explanation of these phenomena is, that nutrition being arrested the results above-mentioned plainly follow; and the obvious tendency of judicious treatment should be to uphold the system by such food as can be easily assimilated.

When Dr. Graves wrote his own epitaph—"he fed fevers"—he made a great move in the right direction; and a recent authority, Dr. Lyons, says:—"The starvation plan of treatment, based on phlogistic pathology, has, when combined with depletion, consigned numbers to the grave."[a]

On this assumption treatment by bleeding and tartar emetic was set aside; and for the very plain reason, that if the disease itself depressed the vital powers of the patient, bleeding and tartar emetic would tend to extinguish them altogether.

The use of mercury was discarded, because its presumed advantages, in cases like these, rest on mere theorising; and, as the theory of inflammation, on which its employment as a remedial agent was based, is by many scientific authorities now deemed unsound, its use is certainly questionable. Moreover, as patients recovered quickly without it, it was deemed inexpedient to inflict on others a cure which, even in the most prudent hands, was often worse than the disease, and which insured to the recipient a lengthened convalescence. It is not at all intended to insinuate that persons cannot mend, and have not recovered when treated by the plans above objected to; they certainly have recovered when so treated; but then many have recovered without any treatment at all.

I am convinced, however, that the proportion of recoveries, and of quick recoveries, under the supporting method is much larger than under any other, and therefore I believe it to be the best and surest mode of treatment.

I now beg to bring before you records of cases of *various* "acute diseases;" and first I shall read, in detail, one of pneumonia:—

John D., aged 21, a young man of exemplary habits, who had been accustomed to perform a good deal on the cornopean, in a brass band, was

[a] Treatise on Fever, p. 199.

taken ill, June 8th, 1862. I found him in bed, with a high circulation, hot and dry skin, complaining of cough, with pain in the chest, and the general symptoms of pyrexia.

Puerile respiration was established in both lungs. Having first directed him to take a purgative, to remove constipation, I advised that he should have five grains of Dover's powder, every third hour, and that frequent turpentine stupes should be applied to the front and back of both lungs.

June 9th.—General symptoms much the same as yesterday. To continue the treatment; beef tea.

10th.—Right lung solidified posteriorly, and crepitus anteriorly in both lungs; expectoration rusty and viscid. To take the following mixture:—Nitrate of potash, 1 drachm; tincture of squills, compound tincture of camphor, and sulphuric ether, of each two drachms; water, 10 ounces. One ounce to be given every four hours; also to repeat the Dover's powders, beef tea, and turpentine stupes to both lungs. Further, wine eight ounces.

11th.—Pulse, 116; respirations, 32; slept better last night than on the preceding; expectoration rusty, viscid, and tinged with blood; urine high coloured and copious; tongue brown and dry. Solidification replaced by crepitus in the back of right lung; mucous râles anteriorly, in both lungs. To continue the treatment as on yesterday.

12th.—Pulse 100, and soft; respirations 28; skin moist; sputa still rusty; mucous râles in right lung anteriorly; respiration re-established everywhere; tongue white; chlorides, in small quantities, present in the urine. To continue treatment.

Evening.—Pulse 106 and soft; respirations 26; sputa less rusty and less viscid; natural breathing re-established, save in the upper third of right lung anteriorly, and crepitus in lowest third posteriorly. Treatment continued.

June 13th.—Slept very well; skin soft; pulse 82 and soft; respirations 26; sputa same as last night, only less in quantity; tongue cleaning at edges; bleeding from the nose; physical signs much improved. Repeat the powders and mixture, one dose of each every six hours; stupes, wine, and broth as before.

14th.—Slept very well last night; pulse 70; respirations 22; skin moist; sputa very copious, less viscid and rusty; bowels costive; face clearer and lighter in colour; urine copious, with lithates; respiration improved; pain in right pleura where there is a slight friction sound.

To have five grains of Dover's powder at night; stupes to pleura twice, and once to upper third of right lung anteriorly (in consequence of slight dulness on percussion). Add 10 grains of ipecacuanha to mixture, and to take a castor oil draught.

June 15th.—Slept well last night; pulse 74; respirations 30; skin

soft; tongue cleaner than at last visit; expectoration natural. Got up this evening.

16th.—Pulse 80; respirations 26; stupe once to right side.

17th.—Chop and ale; wine gradually diminished during the last few days. Cough still remains; continue the cough mixture.

18th.—Ceased to visit him.

From first attack to convalescence, 8 days; under my care altogether, 11 days.

The wine given in this case did not, at any time, exceed 8 ounces in the 24 hours, and it was gradually diminished as the patient improved.

Here, then, was an instance of a young person placed under medical treatment, at an early stage of his illness, before solidification had occurred. The circulation was so high, and the breathing so embarrassed, that had the case occurred three years ago, I should certainly have bled him at my first visit. In fine, it was not one of these *typhoid* cases which some, who object to the *principles* here advocated, feel compelled to treat in *practice*, in a manner very similar to that just now instanced; and the *fact* that he was convalescent and out of bed within *eight* days from his first attack, is, so far as this particular case goes, an argument which cannot be answered.

Next, I beg your attention to some brief notices of a few cases treated by me, in the (Cork) Fever Hospital during the past summer (1862).

Honora B., aged 14, admitted August 31st, with well-defined symptoms of *scarlatina*; was five days ill on admission.

Her treatment consisted in the use of an alum gargle; strong solution of nitrate of silver to the internal fauces; a diaphoretic mixture; five grains of Dover's powder every night; tepid sponging on the surface of the body; drinks of solution of chlorate of potass; beef tea and wine from two to four ounces.

She was convalescent August 6th, seven days from admission.

Richard P., admitted with *scarlatina*, September 13th, treated on the same plan as above. Convalescent September 20th, seven days from admission.

Mary T., aged 7, admitted, with *small-pox*, August 25th; six days ill before admission; had not been vaccinated.

This child had beef tea throughout, but no wine. Her case was, so far as the eruption went, a very severe one; yet she was up September 7th, having been convalescent a few days previously.

Daniel N., aged 19, admitted, with *small-pox*, August 26th. Was five days ill on admission, and had not been vaccinated. The eruption in this case was confluent. He did not get wine, but he got beef tea throughout, and was convalescent August 31st, five days from admission.

Mary H., aged 10, admitted, with *small-pox*, September 4th. Was three days ill on admission, and had not been vaccinated.

On the 7th she got 2 ounces wine; and this was continued every day to the 13th. On the 14th she was convalescent, ten days from admission.

In this, and in the two preceding cases, I had the faces of the patients constantly covered with mucilage of starch, except for a few minutes each morning, when they were sponged off with tepid water to exhibit the true state of affairs, and to apply the starch afresh. Just seven years ago, when small-pox was prevalent in this city (Cork) and neighbourhood, I tried this application in several cases, in the General Hospital in Ballincollig, then under my charge, and I found that in each instance it allayed the sense of heat and itching so much complained of by the patients, and completely prevented their tearing the pustules from their faces.

In the *Dublin Hospital Gazette* for 1856,[a] I wrote a short account of this plan, and its excellence in small-pox, and other skin diseases of an inflammatory nature; and, having used it in the cases just now related, I am able to state that (whether *post hoc* or *propter hoc*) no personal deformity ensued.

Hannah P., aged 15, admitted September 1st, four days ill with what proved to be *erysipelas of the head and face.* In this case the mucilage of starch was also used with good effect. She was treated with Dover's powders, beef tea, and wine from 2 to 8 ounces.

She became convalescent September 11th, after a very severe and distressing attack.

Hannah S., aged 65, four days ill; admitted August 18th with general symptoms of *acute bronchitis.* She had beef tea, and 4 ounces wine daily, from the commencement of her treatment until the 27th, when she became convalescent.

She got no medicine, save Dover's powder and an occasional purgative.

Daniel H., aged 40, seven days ill; admitted August 23rd, with *acute bronchitis.*

This man had turpentine fomentations to the chest, Dover's powder, beef tea, and wine, not exceeding 4 ounces in the 24 hours. He became convalescent August 27th, four days from admission. His attack was not by any means a light one.

Jeremiah S., aged 17, six days ill; admitted August 21st, with pneumonia in both lungs; sputa viscid and rusty. From the 24th, when he came under my care, he got beef tea, Dover's powders, and frequent turpentine fomentations. On the 26th, 4 ounces of wine and Dover's powders, each reduced from 5 to 3 grains. On the 28th, wine reduced to 2 ounces daily. He became convalescent on the 31st. Chlorides present in the urine throughout.

Julia K., aged 15, eight days ill; admitted 20th August. This girl had a severe attack of continued fever, complicated with diarrhea, which set in on the 24th. On her coming under my care, on that day, she got

[a] Vol. iii. (2nd Series), p. 72, &c.

beef tea and Dover's powders; and on the 25th, 4 ounces wine daily, until the 27th, when it was reduced to 2 ounces. She was convalescent on the 30th.

Richard R., aged 17, a stone-cutter by trade, 18 days ill; admitted, with typhoid symptoms, August 24th. This patient had the tongue of that red or raspberry colour peculiar to typhoid fevers. On admission he got a draught, with 2 drachms of tincture of hyosciamus; also 6 ounces of wine, and broth; he further got occasional Dover's powders.

On the 26th the wine was reduced to 4 ounces, and on the 28th to 2 ounces. He was convalescent on the 31st.

Mary M'C., aged 10, five days ill; admitted September 5th. This was a case of *maculated typhus*. From the 7th, she had 4 ounces of wine, and beef tea daily; also three grains of Dover's powder every six hours. 8th, 4 ounces of wine. 9th, 6 ounces of wine. 10th, Dover's powders every four hours.

The wine was gradually reduced to 2 ounces, until the 16th, when she became convalescent.

Eliza O'B., aged 22, four days ill; admitted 20th August. Up to the 24th, when she came under my care, she had been treated by a blister to the chest, and an expectorating mixture; also an aromatic mixture combined with camphor and tincture of hyosciamus.

This case proved a very severe one; there was extensive typhoid pneumonia in both lungs, also long-continued diarrhea, and great nervous depression, with slight abdominal tenderness on pressure. From day to day the case varied considerably, being one day apparently better, and the next worse.

I think it may be classed as one of typhoid fever, though the peculiar papular maculæ were not visible.

On the 24th I directed the camphor and aromatic mixture to be continued, and, in addition, that she should have frequent turpentine fomentations; beef tea; 4 ounces wine; and 5 grains of Dover's powder every third hour; 25th, 8 ounces wine; 27th, a mixture containing sesqui-carbonate of ammonia and chloric ether. September 1st, wine 6 ounces; 2nd, 4 ounces; 3rd, 2 ounces. This day I considered her convalescent.

Denis F., aged 38, a brewery labourer; admitted September 1st, having been previously four days ill. This case proved a very severe one, the patient being in the balance between life and death for several days; and I consider it to have been an example of typhoid fever for the following reasons:—

1. Pulmonic complications; he had typhoid pneumonia in both lungs.
2. Intestinal complications; he had continuous and copious diarrhea; the discharges being of the pea-soup colour and consistence.

3. The typhoid rose-coloured spots appeared September 9th, during the second week of his illness.

4. He had the tongue in the peculiar condition remarked on in the case of Richard R.

5. His delirium was only occasional, and not by any means of an apathetic character; he used to get up and commence to seek for his clothes, but, when remonstrated with, he quietly returned to bed again.

6. His general aspect was not at all dusky, but vivacious, and his eyes appeared glaring and excited.

The treatment consisted in giving him Dover's powders, beef tea, 4 ounces wine, and turpentine fomentations. September 4th, the wine was increased to 8 ounces; 5th, reduced to 6 ounces; 6th, 8 ounces; 7th, 10 ounces; 8th, 16 ounces; 9th, 2 drachms tinct. of hyoscyamus in a draught, with a glass of spirits; 10th, wine 16 ounces, two leeches to each temple, 3 drachms of tinct. of hyoscyamus in two doses of punch; in the evening six leeches behind the ears. 11th, wine 16 ounces, draughts as yesterday, in alcohol. 14th, 1½ drachm tinct. hyoscyamus in a draught of punch, and the same repeated in two hours. 16th, wine 16 ounces; draughts repeated. After this day the wine was reduced to 8 and 4 ounces, and the patient became convalescent September 20th.

Joseph H., a sailor, aged 35, six days ill; admitted September 10th. This I also consider to have been a case of typhoid fever. He had typhoid pneumonia from the outset, and diarrhea. The stools were nearly of black colour, were very frequent, and each discharge nearly filled the chamber vessel.

He had half a glass of punch on admission, and the wine given was from 8 to 16 ounces, and down to 2 ounces daily. He was convalescent on the 23rd.

I could not discover any eruption on the skin; nor had he the extreme nervous prostration and stupor of typhus.

Ellen A., aged 19, admitted September 5th, three days ill. This was a case of maculated typhus complicated with pneumonia in both lungs. The maculæ appeared on the 7th; on the 8th her face became suffused and dusky, and the tongue dry and brown.

She was treated on the supporting method; got wine from 6 to 16 ounces, and down again to 6 ounces. She was convalescent on the 19th.

Timothy H., aged 35, a brewery labourer, 14 days ill, was admitted September 5th, with well-developed *maculated* typhus.

This case, which was accompanied by the lowest possible amount of nervous depression, was treated by Dr. M'Evers,[a] on the supporting method from the outset.

The day of admission he had 8 ounces wine, 2 ounces spirits, beef tea,

[a] My valued colleague, by whose kind permission I am authorized to make use of this and the following case.

and turpentine fomentation to the chest. The frequent complication of pneumonia occurred here also; and on 8th the patient is noted as having on the surface of the body, "maculæ, petechiæ and purpuraceouss pots."

He was stimulated in every possible way, external and internal, and, after a hard fight, became convalescent September 19th, having consumed, besides beef tea and medical stimulants, 216 ounces wine, 89 of brandy and spirits, and 10 pints of porter, from September 5th to September 30th, the date of this note.

The external stimulants were also formidable; turpentine fomentations, sinapisms, and blisters were applied, as the unceasing attention and anxiety of his medical attendant directed; and I have not the smallest doubt, after careful watching of this case, that, had not the supporting method been adopted from the outset, Timothy H., could, humanly speaking, never have recovered.

Eliza K., aged 24, six days ill, admitted September 10th, with unmistakable diphtheria and cynanche tonsillaris. Dr. M'Evers (under whose care she was), besides making her use a liniment of ammonia and stuping her throat, gave her two pints of beef tea, and 8 ounces wine. He also caused the muriated tinct. of iron to be applied to the internal fauces, gave her a gargle of the same tincture (2 drachms to 1 ounce of water), and 20 drops of the tincture in water, every four hours.

On the 14th the wine was reduced to 4 ounces; on the 15th the tincture was stopped; and on the 18th she was convalescent. While in the convalescent ward she threw up the diseased membrane, which she very clearly described as being "like the cover of a neats' tongue when boiled."

I suppose this may be considered an instance of what Dr. Jenner calls "the sixth variety," the asthenic form of diphtheria.[a]

On looking over the cases of continued fever noted in this paper, I think there is some fair ground for demurring to Dr. Jenner's theory of typhus and typhoid, proceeding from totally different poisons. While the typhus and typhoid types were clearly defined in some, yet in others the symptoms were so mixed as to lead to the conclusion advocated by Dr. Henry Kennedy:[b] that while the two types could generally be distinguished one from the other; yet, that to obviate all the difficulties surrounding the question, they should be considered as the result of a common poison. In the latter of his two papers, quoted below, he gives such a number of cases of the doubtful kind that there appears to be much force and originality of thought in the saying of Miss Nightingale,

[a] Diphtheria, its Symptoms and Treatment, p. 29.

[b] On Typhus and Typhoid Fevers, as seen in Dublin (read before the Royal Med. Chir. Soc., London, in May, 1860), and also, Further Observations on Typhus and Typhoid Fevers as seen in Dublin, Dublin Quart. Journ. Med. Science, Vol, xxxiv., p. 50., &c.

that we should not look on diseases as separate entities, like so many dogs and cats, but as varieties, or different phases of the same morbid change or condition.[a]

The alternative view—that any two of the fever poisons may coexist in the same individual, and each exhibit its special phenomena—appears also to be a probable explanation of these cases, especially if we can conceive that one poison may, to some extent, neutralize the other.

However this may be, I do not wish to convey the idea that typhus and typhoid (or enteric) fevers, are synonymous terms for one disease. The distinction is too evident to allow such an opinion; although there are yet many highly esteemed and observant practitioners who, looking on the clearing out of the bowels as the all-important object in typhus, and, at the same time, refusing to admit the existence of enteric fever, continue, in unmistakable cases of the latter, to purge the patients, and so in many cases retard their cure, while in some they prevent it altogether.

I shall conclude these cases by mentioning, that during the last two years I have treated various persons in private life on the principles here advocated, with considerable success. I particularly remember an instance of acute hydrocephalus, and a very distressing instance of cynanche tonsillaris in a clergyman; besides several others occurring in the ordinary course of things. When affected with pneumonia myself, about 18 months since, I was treated in like manner and with like success.

Now, while I utterly repudiate the old routine practice pursued in every case of acute disease—I mean bleeding, lowering treatment, and mercurialization—I equally repudiate the opposite extreme, the treatment of all acute cases with alcohol in large quantities. This practice, with the late Dr. Todd, seems to have been pushed to such excess that it became a routine, just as much as the ancient practice of bleeding for every disease; a plan still ruthlessly pursued with sick horses and cattle in our country districts.

In the cases noted in this paper there has not been—I hope it will be admitted—an undue administration of stimulants or alcoholic food. In some there was none at all, in others very little, and in all the dose was regulated by the symptoms most pressing at the time. The principle was neither entertained or acted on, that stimulants, in large quantities, and, as a matter of course, ought to be given in every acute case. But it is

[a] Since the above remarks were written, I had two cases:—One (W. D.), a lawyer's clerk, having all the prominent symptoms of typhus, except the maculæ; and a second, which is only now (December 10th, 1862), convalescent, after nearly eight weeks' illness. In this instance the patient (W. J.), a gentleman, aged 19, had all the plain and undoubted evidences of typhoid fever, except the spots. The first case was severe, but recovered under the treatment advocated here. The second was frequently believed to be dying; for weeks he may be said to have lived on wine, brandy, and spirits, proving, incontestably, that alcohol is *food*. To him it was *aqua-vitæ*.

very respectfully submitted, that the principle of support *versus* depression and mercurialization is a surer, shorter, and therefore a vastly better principle than the reverse.

At the same time it is not intended to deny—for it cannot be denied—that the plan, against which I argue, has, in the absence of clearer knowledge, had considerable success. But so it is with almost all modes of cure, from the time of Hippocrates to the present; at least so say the respective advocates of the various methods.

But in some respects the difference is more imaginary than real. Several, who wholly object to the *principles* laid down in this paper, find themselves compelled to treat acute diseases by support, to a greater extent as compared with their practice in former years. They say that the type of disease has altered; and that as inflammation is not now so strong as it was 20 or 30 years ago, so it does not now require the treatment adopted by a past generation.

But is this the case? is it wholly or only partly true? I think only partly.

We are very apt to fall into error respecting types of disease, when we take our impressions of them from the way in which they exist, or are said to exist, in London and other large cities.

The diseases in those towns are often quite different from those occurring under the same names in country districts.

While a London or an Edinburgh physician may say that acute disease is now more typhoid in its character than formerly, if he came into some of the country dispensary districts in this (Cork) county, which I could name from personal knowledge, he will find pneumonia just as acute now as it was represented to be by Cullen and Gregory; while in some parts of this (Cork) city it will be found to be of a depressed character.

Thus it is in a large city like London, that in the course of a few years the town extends, and so does the dirt; the population doubles; families are crowded together; the necessaries of life become more difficult to be had, and of inferior quality; and the habits of the people are such that typhoid disease results, or, if you prefer it, ensues; whereas, if one of our peasants, living in a miserable mud cabin, gets pneumonia, he, by the aid of external country air, and the disinfecting turf-smoke of his residence, overcomes the effects of the pig's society inside, of the dung-heap and cess-pool outside, and, more marvellous still, of the surgeon's lancet, and soon recovers from, what most will admit to have been, as severe an acute attack as his father or grandfather ever had.

Further, I am ready to concede that essential fevers, and other acute diseases, are—not sthenic, or asthenic, but—more asthenic at sometimes than at others: for instance, in the recent epidemic fevers in this city (Cork, summer of 1862), the cases admitted to the Fever Hospital were, in general, of a much more depressed class than those admitted last year,

and so required much more wine than their predecessors. But that will not account for the fact, that in the very respectable library of registers preserved in the institution just referred to, during the last 60 years, the frequent recital of deeds of blood comprised in the quiet term "venesectio" —that mandate which, it is to be feared, sealed the doom of many a patient—occurred largely, and, as a matter of routine, half a century since, whereas a lancet is now a most unusual visitor in the same place. On looking over one of the registers referred to, that for 1818, taken at random from among a large number, I find the number of patients for that year was:—males, 2,779—venesections, 571; females, 1,424—venesections, 485; that is, more than 1,000 persons were bled out of about 4,000.

In 1861 there were 608 persons treated in the same hospital, but there were not any venesections during that or the four preceding years.

The venesection cases of 1818 are recorded as mostly having "fever from contagion;" next in frequency "synochus" is mentioned, and also pulmonary affections. The cases treated there comprised then, as they do now, all acute diseases; that is, not only what we now call typhus, typhoid, and relapsing fevers, but all internal local lesions in which febrile symptoms were prominent.

A *partial* change or *moderation* in the use of blood-letting may be accounted for by saying, we are now in an asthenic cycle of disease; but as men are as strong now as men were 50 years ago, and as depressed types of disease are at all times to be found in debilitated or unsound constitutions, as in badly fed workhouse children, it cannot be maintained that all over the kingdom, for several years together, in good and bad constitutions, under all conditions of food and temperature, acute disease has assumed so low a type as to obviate the necessity of blood-letting, which was formerly not only the panacea for all ills, but even the periodical requisite of those who were specially remarkable for health and energy. The immediate relief following general bleeding in many cases, no doubt, kept up its use; and many a physician got a good name and blessings from the patient whose recovery he retarded, or perhaps altogether prevented, by that imprudent *indulgence*.

It is very curious to observe how opinions, which are deemed modern, turn out to be ancient enough. This bleeding and supporting question is now frequently referred to in our literary periodicals; thus, in an article on "Dean Hook's Lives of the Archbishops of Canterbury," in *Blackwood's Magazine* for July, 1861, speaking of the course of education pursued in the College founded by St. Augustine at Canterbury, the writer says:—"There were brave attempts made also to teach what we now call special subjects; and useful knowledge, music, astronomy, natural philosophy, and medicine, had each their turn. The music was good of its kind; of the natural philosophy and astronomy it can only be

said that they kept pace with the theories of the day; and medicine is still so much an experimental art among ourselves, that it seems quite possible that our own theory and practice may appear barbarous in the light of future discoveries, as that of the seventh century now does to us. If Archbishop Theodore declared it to be very 'indiscreet and unskilful to bleed a young lady on the 24th day of the moon,' he was so far a step in advance of most surgeons of the passing generation, who took the lancet ruthlessly in hand every day throughout the year; while modern science carries out the Archbishop's idea more consistently, and pronounces it indiscreet and unskilful to bleed at all in 99 cases out of 100."

The public importance of this question is further instanced in the following extract from an article on "Country Doctors," in the *Saturday Review* for September 7th, 1861:—

"It is significant that the equivocal name 'leech' has been conferred in common on the human and the reptilian practitioner; and if you were abroad you would realize its meaning to the letter." The writer of the article then refers to the death of Count Cavour, who had been bled six times in fever, and proceeds:—"In England you have no such fate to fear, even the country doctor has, in most places, got beyond the lancet. . . . When he administers a few thumping doses of calomel, and brings you to death's door, he experiences a feeling of pardonable exultation at being the possessor and recognised master of so potent a spell."

In the recently published work entitled *Clinical Medicine,* by Dr. Gairdner, now Professor of Medicine at Glasgow, the writer, who thinks that in some cases of pneumonia bleeding may be of use, that very small doses of tartar emetic may do in others, observes of his own practice:—"Calomel with opium (indeed mercury in any form) has been very little employed, having been given only in obstinately continuing condensations, and then only as an experiment, with great caution, and with, as I think, little positive result."[a] He does not look on alcohol as *food*, but as a *tonic* or *stimulant medicine.*

That it is *food* is, I think, well maintained in an excellent paper, entitled, "Does Alcohol act as Food," in the *Cornhill Magazine* for Sept., 1862, where the writer says:[b]—"*Food*, I take it, means anything which, taken into the body, will, unaided, keep it from perishing so quickly as it must in the absence of all sustenance;" and then proves his case by showing that, in certain diseases, alcohol, and that only, has sustained life for a lengthened period of time. Dr. Gairdner winds up his experience of pneumonia by this good advice:—"In all cases of doubt and difficulty, I have uniformly adopted the principle that nature is to be trusted to a great extent; believing that patients will recover much better and sooner under no active treatment at all, than under a routine treatment blindly enforced; or, in other words, that nature is a better manager than a

[a] P. 54. [b] P. 322.

bungling physician, who has always an inexorable system in hand to control her operations."[a]

But perhaps some one will say—what is the treatment recommended in the foregoing remarks but "a routine treatment, blindly enforced"? Does not the constant repetition of the phrase "Dover's powders, beef tea, and wine," in the great variety of acute cases just enumerated, bring the suggestions contained in this paper within the censure of Dr. Gairdner? With all respect, I think not. The objection lies really against the course of nature, or, more correctly, of disease, since several of the prominent symptoms (*ex. gr.*, those included under the term pyrexia) are in fact *routine* symptoms, being found in diseases of different name, character, and severity.

Thus, on the principles of treatment applicable to most acute diseases, laid down in the opening part of this paper, the use of Dover's powder as a medical agent, cannot be called routine practice, any more than that of any other diaphoretic mixture. It is not one drug, like tartar emetic or mercury, but a most excellent combination; and is useful because of the ease of its administration, and facility of preparation in proper doses.

It was not given in all cases, nor in the same doses in any number of cases, similar or dissimilar; nor was it continued for the same length of time in any number of instances. The drugs composing it could have been made up in half a dozen different forms, into mixtures of different sizes, colours, and tastes, into pills, powders, and boluses; in which cases its use could scarcely be called routine. Nor was the use of wine routine either, as I have already fully shown; and I have previously explained that beef tea was given on the principle of support *versus* depression, and in quantities proportionate to the circumstances of each case. All this is surely a very different thing from the invariable treatment of pneumonia in the country districts *now*, and in the large towns 20 years ago. First, bleed your man *ad deliquium;* then either give him tartar emetic solely, mercury solely, or both; and let the patient, if he can, struggle against his disease, starvation, and all these depressants. Which of these plans is "no active treatment at all," and which is "routine treatment blindly enforced?"

Our great philosopher,[b] writing of the "conjectural" in the art of medicine, observes:—"The lawyer is judged by the virtue of his pleading, and not by the issue of the cause. The master of the ship is judged by the directing his course aright, and not by the fortune of the voyage. But the physician, and perhaps the politician, hath no particular arts demonstrative of his ability, but is judged most by the event; which is even but as it is taken; for who can tell if a patient die or recover, or if a state be preserved or ruined, whether it be by art or accident?"

[a] P. 55.

[b] Lord Verulam, Advancement of Learning, Book ii.

Judge this plan by the *event;* it has been shown to be successful at any rate in several serious cases; and so long as those who object to it on principle admit the force of ordinary personal testimony, they cannot controvert the issue of the cases registered in this paper.

Let not any one who has tried it in one instance only, and found it unsuccessful, reason against it; it is illogical and invalid to reason *a particulari ad universale.*

Nor let any one assert the success of this system to be contrary to experience when he has never tried it, and resolves that he never *will* try it. He may plausibly argue against the *theory* here assumed; but unless he has been present at the treatment of the preceding cases, or of others similar, and has observed no such result as is here affirmed to have followed, he is, I respectfully submit, entirely disqualified, by all the rules of reasoning, from making any objection to the *practice* laid down in these observations.

To adapt to this subject the well-known lines of Horace, I may say, in conclusion:—

"——————— Si quid novisti rectius istis
Candidus imperti: si non, his utere mecum."

On the Treatment of Acute Diseases. By Professor O'Connor, M.D.

The followers of Pythagoras had two classes of opinions:—The one called esoteric, for the thoroughly initiated disciples; the other exoteric, which were adapted to the understandings of the general public. I have often wished that in medicine we had a similar practice, that the profession might be thus saved the scandal produced by the contradictions found in medical writings on some of the most vital questions. In the earliest times Hippocrates, the father of medicine, saw and lamented this evil, which he refers to in the following words:—"The whole art is thus exposed to much censure from the vulgar, who fancy that there is no such science as medicine, since in acute diseases practitioners differ so much amongst them, that those things which one administers as thinking it the best that can be given, another holds as bad." One would suppose he was referring to the discussions which take place at the present day on the treatment of acute diseases; one party contending that these are to be treated by stimulants and sedatives, another by mercurials and antiphlogistic measures. I have been led to make these remarks by having heard the paper on this subject read, at the last meeting of our society, by Dr. Belcher, which, although written with great ability, contains doctrines that should receive the most mature consideration before being adopted by the profession.

Lest I should misrepresent Dr. Belcher's views I shall make no further reference to his opinions than to state, that he appears, by his practice, to

recommend the treatment of all diseases, inflammatory as well as essential fevers, by the one common form of sedatives and stimulants, a practice put forward lately in the writings of a few men of eminence, and which has been imitated by many of the junior members of the profession, I fear to the great injury of their patients.

It might be interesting to inquire why, after so many centuries of careful observation by the most enlightened minds, medical opinions should be still so unsettled on subjects of the most vital importance. The reason might be found in the impossibility of establishing any truth in medical practice by a strict induction. We should not exclude from our reasoning the various other causes besides medicines, which either retard or aid the cure—the influence of age, climate, season, the previous habits of the patient, his constitutional idiosyncracies, the constitution of the atmosphere as modifying the type of disease; above all that great power the *vis medicatrix naturæ* which aims at a restoration to health in every curable disease; and still, unless we can accurately estimate the influence of these forces, we can never be certain what share a medicine had in the cure of a disease, or whether the cure did not arise in spite of the treatment. Accordingly every new doctrine in medicine, whether the offspring of vanity, weakness of judgment, dishonour, self-interest, the love of notoriety, or of the gain which often follows it, or whether it arises from higher motives, is, in every case, introduced to the public, sustained by a number of successful cases. In this respect the quack and the scientific physician stand before the world on equal terms.

Still, I am sure the contradiction is more in medical writings than in medical practice. Men of good understandings, though holding opposite theories, seldom differ about the treatment of a particular case. When the life of an individual is at stake men, unconsciously, bend their theories to the true practice.

A reference to the writings of the father of medicine, Hippocrates, will show us that the profession were never left without a pure and true light to guide their treatment in acute diseases. In reading the opinions of this great man, we cannot help being struck with their moderation, the absence of all theorizing, his counselling a special treatment suited to each case, irrespective of all preconceived ideas. In all of which he contrasts most favourably with some few writers of the present day, who urge the adoption of a general plan of treatment for all acute diseases, and that, a treatment in direct opposition to the instincts of the patient and the opinions of the vast majority of the profession.

In speaking of the diet of the sick, Hippocrates says:—"It should be as different from that of the healthy as that of the healthy is different from the food of wild beasts." Is this distinction counselled by these gentlemen who recommend an amount of stimulants and nutritious food in the most inflammatory complaints, that would bring sickness on the most

healthy. Again, after giving directions for the preparation and administration of light diluent drinks, lest this system might be carried too far, he adds:—"In the constitution of man abstinence may enervate, weaken, and kill; and there are many other ills different from those of repletion, but no less to be dreaded, arising from deficiency of food. For one must aim at attaining a certain measure, and yet this measure admits neither weight or calculation of any kind by which it might be accurately determined, except it may be the sensation of its bad effects. Wherefore, we should strive to learn this accurately, so as not to commit small blunders, either on one side or the other." In this the physician is told that neither stimulants or starvation is the proper treatment for acute diseases, but there is a golden medium between these extremes which it should be his sole care to attain. On the same subject he says:—"A weak person is in a state nearly approaching to one in disease, but a person in disease is more likely to suffer if he encounter anything that is unseasonable." Speaking of the use of wine, his instructions are equally guarded:—"We must, in each case, determine whether it is to be given or not, and at what time, and of what quality." Again, "in protracted diseases pay attention to the exacerbations and remissions of the fever, so as to avoid the times at which food should not be given, and when it might be administered without danger." His instructions about bleeding are equally moderate, counselling it only "in acute affections, if the disease appear violent and the patient be in the vigour of life, and that he have strength. Abstract blood more or less according to the habits and ages of the patient." I am sure that principles such as these have guided the practice of the great body of intelligent physicians in every civilized country, notwithstanding the jarring of schools, both ancient and modern. Here we have neither the antiphlogistic nor stimulant plan of treatment recommended as a general practice, but the physician is counselled to study each case with care and without a rash confidence in himself, and then to determine what measures he should take for the restoration of his patient.

Seeing how strongly the writers to whom I have already referred condemn the use of all depletion in inflammatory diseases, substituting its opposite, namely—the strongest stimulants—and that they sustain their views by an appeal to experience as well as theory, it has occurred to me that some light might be thrown on this subject by referring it back to nature, as to a supreme court of appeal. All writers agree that in all acute diseases nature makes an effort at cure, at again restoring injured parts to their healthy condition; without this provision our organization would be very imperfect indeed. In the union of fractured bones, in the separation of sloughs and gangrenous parts, and of the dead bone in necrosis, we see the *modus operandi* by which these measures of safety are carried out by nature. Though not so obvious to the senses, nature's

cure is accomplished with equal wisdom in inflammation of the liver, lungs, or brain. If it be the design of Providence that injured organs should be restored to health by a paroxysm of disease, it follows that this object will be attained with the utmost wisdom, and that the patient will be placed in circumstances most favourable for its attainment. We find that, as soon as an inflammation of any important organ sets in, muscular debility soon follows, attended with prostration of mental energy, and thus the patient is, of necessity, condemned to repose of mind and body, just as we place a broken limb in splinters; and the physician as carefully enjoins the one as the surgeon practises the other. Next, the stomach, for the wisest reasons, is placed in sympathetic connexion with every organ of the body, even with the exercise of the moral and intellectual faculties, and at the commencement of inflammation all appetite for solid food ceases, and is replaced by a desire for diluent drinks. This is so invariable that it cannot be without its purpose, any more than the increase of appetite which attends on bodily labour, or on the rapid development of the frame. In the one case nature wishes to diminish, in the other to increase the quantity of blood. In the former all fresh supply of blood ceases, while diluents assist the liver, skin, kidneys, and bowels to remove the various excrements with the least distress to these organs. There is thus, in the first few days of an acute inflammation, a gradual diminution of the quantity of blood in the body, to a grater extent than the boldest phlebotomist would practise: just as the captain of a ship, when the storm arises, throws overboard whatever might encumber the movements of his vessel. It is impossible not to think that nature diminishes the quantity of blood, and renders it less rich in its quality to save not only the organ affected, but also the others which are endangered by the great perturbation of the system arising in an inflammatory fever. Why not respect the suggestions of nature in this as well as in the other instance? Should we not avoid opposing nature by the too early use of nutritious and stimulating food, even though we may not think it necessary to anticipate her by active depletion? It might be asked, when heat of skin and a quick pulse also attend on inflammation why should we, by depletion, diminish the one or the other; or, if they are parts of a salutary process, should we not rather increase them by the use of stimulants? To this it might be answered, that nature acts by general laws; and though in the main these attendants on inflammation might be salutary, in individual cases they might be in excess and require repression at the hands of the physician. The monsoon, like many other phenomena in nature, while it brings health to the many by purifying the atmosphere, brings destruction to some individuals. No physician attempts to reduce the pulse or heat of skin to the normal standard during inflammation; still he will, in many cases, be called on to moderate the one and the other. If he believes that the excitement is

so great as to endanger life, or to implicate other organs besides the one originally affected, it will be his duty to moderate it by abstraction of blood, by active purgative, or medicines calculated to weaken the force of the heart's action. The manner in which the physician is bound to follow the suggestions of nature is well illustrated in the practice of midwifery. Never is he more directly employed in saving human life than in this branch of the profession; still it is only rarely that active interference on his part is necessary. In this process—the most uniform, perhaps, in the animal economy—nature sometimes fails to accomplish her object; and we are successful in proportion as we justly appreciate this failure and follow exactly nature's mode of action in normal cases. When the membranes are too strong we rupture them at the time we find they would break in ordinary cases. We know the time the placenta would usually be forced away, and we remove it if delayed beyond that period. If internal hemorrhage be taking place, we stimulate the uterus by external or internal irritation, because we know that when the uterus was distended by clots it would itself contract and expel them. We use artificial means to extract the child, when we believe the efforts of nature are in vain In all these cases we act as nature would act, and where she would act. It is the just appreciation of the necessity for interference and following the *modus operandi* of nature which distinguish the enlightened from the ignorant physician. If we watch the proceedings of nature in parturition are we not equally bound to attend to it in inflammations? Is it not the voice of nature that calls for cooling diluent drinks, and rejects the nutritive food and stimulants so agreeable to health; which calls for the refreshing breeze to cool the patient's body, and urges him to cast off his usual covering; shall we, in spite of these suggestions, continue to force on him food and drinks abhorrent to instincts, given him by Providence to aid his restoration to health?

Those who advocate the stimulant treatment in acute diseases are not so original as they may wish to think. Sydenham had to contend against it in his day; and it is still the favourite practice among the poor and ignorant in the country, who think a breath of cool air, a cold drink, clean sheets, or washing the skin, would bring ruin on their patient, and who never fail to ply them with whiskey punch "to bring out the disorder." Thus the false philosophy of the rich of one generation descends to the poor of the next, like their cast-off clothes and antiquated fashions. This practice was brought to its highest pitch of absurdity by the celebrated author of the *Brunonian* theory; but he, at least, had the merit of practising what he inculcated, for he always commenced his lecture by taking a glass of brandy and 60 drops of laudanum, and repeated the dose two or three times before its conclusion. It would be well if the modern advocates of this theory would try a full dose of their own remedy before administering it to their patients.

It is natural to expect that practitioners of the school referred to

should altogether object to bleeding. For them to practise it would be as irrational as throwing fuel and water into a fire at the same moment. Accordingly, we find it objected to—first, "because it could no more stop the progress of inflammation once begun than it could check scarlatina or the small-pox." This is altogether a gratuitous assumption. We have positive proof that fevers run a definite course. We have equally positive knowledge that nothing is more uncertain than the duration of an inflammation, or the extent to which it may spread. As these conditions vary in different cases nothing but experience could determine whether they are affected by treatment. It is true, that "when exudation once takes place, nothing but a natural process will reorganize and remove it;" and the sole question in treatment should be, what state of the general circulation would most facilitate this process, and prevent the further spread of inflammation. Whether there is not a medium state between plethora and debility most conducive to the reparatory process, and whether we may be justified in diminishing the excitement in one case as much as we are in stimulating in the other. The opponents of bleeding admit the necessity of the latter; how can they ignore the former. Judicious bleeding does not lower the strength in the one case, more than nutritive food and stimulants increase the inflammation in the other.

We should also consider that our treatment is not entirely directed to the condition of the inflamed organ. Thus, if half one lung becomes suddenly impervious to air, and the circulation in it is arrested as in pneumonia, or an entire lung becomes compressed, as in pleuritis, the cavities of the heart become over-distended, the brain and other organs become endangered. Blood-letting alone can speedily remedy this evil, and restore these organs to a healthy condition, though it should not be able to arrest the inflammation in the part first affected. As theory cannot completely solve this question, we are again driven to the test of experience; and we have for its advocates the great body of medical practitioners, from Hippocrates to the present day, differing from each other solely as to the extent to which this treatment should be carried. I shall repeat Hippocrates' maxim:—"Bleed in acute affections, if the disease be violent and the patients be in the vigour of life, and that they have strength; and then abstract blood more or less according to the habit and age of the patient." Is not this the doctrine and practice of all the sound practitioners of the present day? who, distinguishing essential fevers from inflammation, consider that the former require support with very moderate, if any depletion, while the latter requires depletion with very little support; and that occasionally the treatment might have to be reversed, as when inflammation arises during the progress of fever, or when debility accompanies inflammation from its commencement, or arises subsequently. Our principle should then be to direct our practice, irrespective of theories, to the individual case, the cure of which is the paramount object of both.

A Contribution to the Normal and Pathological Histology of the Kidneys. By V. RASMUSSEN, Candidate in Medicine and Surgery. Translated from the *Bibliothek for Læger* for April, 1862. By WILLIAM DANIEL MOORE, M.D., T.C.D., M.R.I.A., Honorary Member of the Swedish Society of Physicians, of the Norwegian Medical Society, and of the Royal Medical Society of Copenhagen.

ALTHOUGH it cannot be asserted that cellular pathology is the only true and absolutely valid doctrine, inasmuch as it cannot be denied that, carried out to its ultimate result, it too much loses sight of generalization, and does not knit together all details into a higher organic unity, yet it may be said that it is the culmination of our present knowledge. Even the Vienna School, which, with its postulates and arbitrary interpretations has so long ruled science, has been obliged to bend before the numerous incontrovertible facts, supported on an exact genetic observation—for precisely this is the main strength of the new doctrine—and has quite recently begun to forsake the beaten path of authority and tradition, to turn more to the objective matter-of-fact method. Cellular pathology teaches us to separate the several tissues which compose the organs, and, in morbid processes, to investigate which tissue is the affected one, in order subsequently to estimate and name the disease; hence it is clear that a knowledge of these tissues, and of the elements composing them, as well as of the mode in which they are arranged to form the several organs, is a necessary condition for the comprehension of their pathological changes. As it is, therefore, my object in these pages to give a sketch of the histological relations in certain renal diseases, it will probably be found advisable, first, to direct attention to the normal anatomical conditions, especially to such as are important to us in the appreciation of morbid processes in these organs.

The Renal Vessels.—The striking difference found in the latter stages of the so-called Bright's disease, between the perfectly anemic cortical substance and the strongly hyperemic pyramids, led Virchow to institute some investigations respecting the vascular distribution in the kidneys, as this frequent and almost constant pathological condition was manifestly opposed to the doctrine first promulgated by Bowman, and subsequently adopted by Kölliker. As is well known, this was to the effect that all the blood, brought through the renal artery to the kidneys, with the exception of that circulating in some inconsiderable branches to the investing connective tissue, does not reach the veins without having passed the Malpighian bodies. That the latter are supported everywhere upon the extreme ramifications of the renal artery (vasa afferentia), and from them emerge the efferent vessels, which are differently disposed of. That while, namely, by far the greater part of them pass, after a short course, into an

ample capillary net-work encircling the convoluted tubes, those springing from the glomeruli lying nearest to the pyramids pass over into the so-called arteriolæ rectæ. That the latter, which are of considerably greater calibre than the other efferent vessels, enter into the pyramids, and run between the straight canals, dividing, at acute angles, into smaller branches, running, however, in the same direction—which finally, towards the apex of the pyramids, pass into an extended capillary net-work; during this course they, at the same time, give off branches inwards, which form a net around the straight canals, anastomosing with capillaries of the cortical substance.

In opposition to this theory, Arnold, Hyrtl, and Leydig assume that the arteriolæ rectæ spring directly from the main branches of the renal artery. Virchow's views (*Archiv für Pathol. Anat. und Phys.*, Bd. 12) combine both these opposite propositions; inasmuch as, according to him, the arteriolæ rectæ are formed—1, of branches issuing directly from the renal artery (main trunks—arcus vasculosi); 2, of vasa efferentia of the glomeruli situated next to the base of the pyramids; and lastly—3, of capillary outrunners from the cortical net-work. According to Virchow there is, at the boundary between the cortical substance and the pyramids, a neutral space, where the arteriolæ rectæ still form glomeruli, while the vasa efferentia send out lengthened off-shoots, which in general pass over into the capillary net-work of the pyramids, but sometimes both to it and to that of the cortical substance; and as the latter afferent vessels, proceeding from the arteriolæ rectæ, are most frequently very short, it may seem as if the arteriola recta itself, which, in reality, only runs past the glomerulus, were the efferent vessel. As to the distribution of the large vessels, the larger branches of the renal artery appear, through the columnæ Bertini, constantly giving off branches, which most frequently are regularly dichotomous, and which in flat arches (arcus seu fornices vasculosi) shoot through the outer part of the base of the pyramids, and thence obliquely up through the neutral zone, in order, finally, about the middle of the pyramids, to meet—though without anastomosing—with a corresponding branch from the opposite side, and to ascend in the cortical substance. These arcus vasculosi are to be considered as trunks, which now again give off branches, whereof by far the greatest part proceed from the convexity, and ascend directly or obliquely in the cortical substance between its several lobes. These branches, the so-called interlobular arteries, give off afferent vessels, which, as Virchow first demonstrated, do not arise at an acute angle, but in general form more or less backward curved arches; that is, in irritation of the papillæ, a circumstance which we shall subsequently see is of essential import. From this view it therefore follows that the cortical substance and the pyramids have a circulation independent of each other; the pathological state just mentioned, which, according to an older theory respecting the

vascular distribution in the kidneys, was inexplicable, has now found its natural simple explanation: if the blood, from any cause, becomes obstructed in its free entrance into the cortical substance, a compensatory congestion of, or determination to, the pyramids takes place, and *vice versâ*.

The veins, of course, pass, for the most part, into the renal veins, but the peripheric veins are at the same time connected with the renal capsule, and through it with the venæ azygos and hemi-azygos, a circumstance which is easily demonstrated by drawing away the capsule in cases of venous hyperemia; the connexion takes place through the stellulæ Verheynii, which in such cases are distinctly apparent; and, on more accurate examination, the torn branch is easily found.

Renal Parenchyma.—The whitish striæ in the pyramids, which give the latter the characteristic feathery appearance, correspond to ramifications of a single urinary canal opening upon the papilla. These, the so-called Ferrein's pyramids—properly speaking this name does not belong to them, but in modern nomenclature they are most frequently so termed—everywhere tend, and also in the columnæ Bertini, towards the cortical substance; but in entering the latter the several canals, hitherto running parallel to one another, curve in a centripetal direction towards the sides, and make, in irregular windings, for a glomerulus; meantime the original pyramid becomes more and more slender; and, after having passed through about two-thirds of the cortical substance, it ceases, and thenceforward to the surface of the kidney only tortuous canals are found. In this mode, therefore, conical figures of straight canals also are formed, with the base towards the great pyramids; and it is, properly speaking, to them that the name Ferrein's pyramids applies. Such a pyramid, with its series of tortuous canals, constitutes a renal lobule. A section, carried perpendicularly to the axis of the latter, will therefore present very different appearances in the several points: towards the surface of the kidneys purely tortuous canals; internally, towards the pyramids, predominatingly straight ones; and transitions between these two. When we have, in the usual manner, by a section parallel to the axis, obtained a smooth section of the kidney, we often see these lobuli bounded by reddish striæ (vasa interlobularia); however, these vessels do not form actual boundaries, they are only like isolated partitions, met with at a distance from one another; but, where these are wanting, the lobules, and therefore the tortuous canals, are in immediate contact with one another. The case is here the same as with the acini of the liver, with which, on the whole, the lobules of the kidney may be compared, for there, too, the net-works of hepatic cells are separated from one another only by the vessels and the connective tissue accompanying them.

This is, so to speak, the coarser anatomy of the kidneys, so far as it goes, which, on more accurate investigation, is easily observed in the smooth

section. In addition it may be stated, that on both sides of the interlobular arteries glomeruli are seen as small clear points, arranged in series. The renal parenchyma must, in order to be called healthy, not allow the individual urinary canal to appear too prominently, and it must be half transparent. Kidneys presenting such perfectly healthy parenchyma are very rarely seen on section, when death has not occurred suddenly, or, at all events, after a very short illness; most frequently there is a change, though only a slight one, of the epithelium in the renal canals, especially the tortuous ones, which gives them a dull whitish-grey appearance, and makes their outline appear more marked.

As to the more minute examination of the urinary canals and glomeruli, their membrana propria is in general said to be structureless; and this is in fact the case, although Beer (*die Bindesubstants der menschl. Niere*, Berlin, 1859) says he has demonstrated minute ramified corpuscles of connective tissue in them; here and there nuclei are found enclosed in the doubly contoured membrane. The tunicæ propriæ, freed from the epithelium, have a tendency to form fine folds, especially in a longitudinal direction, a circumstance to which Kölliker has already called attention; and this it is which has led Beer to assume the existence of corpuscles of connective tissue in them, the more so as the carmine, used in the investigation, readily adheres to such points, and thus makes the appearance still more deceptive. The epithelium on the inside consists of polygonal cells, with a large round, sharply contoured, granular nucleus, which, especially in children, is remarkably large, almost completely filling up the cell; rarely do we observe cells with two nuclei indicating a division. That the cells are polygonal, and in general have no outlets, can be proved by treating them with a weak solution of nitrate of silver and solution of salt, a method which has recently been employed by v. Recklinghausen, and whereby he has shown, that in all kinds of epithelium, there is a connective tissue between the individual cells, which is coloured black by the precipitated chloride of silver. By this mode of treatment the epithelium at present under our consideration exhibits acute, black, polygonal boundary lines; where it has been supposed that ramified epithelial cells have been met with in healthy kidneys, this appearance may possibly depend upon the fact, that portions of the connective substance have remained adherent, as fine filamen ts,to isolated cells. Such isolated cells, furnished with outlets, were seen by Beckmann in slight forms of nephritis. This connective substance is tolerably strongly developed in the renal epithelium, on which account the cells adhere firmly to one another, and, indeed, often separate in complete tubes from the membrane. The contents of the cells are finely granular, and are readily clarified by acetic acid; the enveloping membrane is exceedingly fine and slender. Wittich has, it is true (*Virchow's Archiv*, Bd. 10), endeavoured to deny the existence of the

membrane, considering these epithelial cells, which he connects externally with the urine, and internally with the blood-vessels, as nuclei surrounded with a highly albuminous protoplasm. Although the membrane can no longer be considered as essential to the formation of a cell, it is easy to prove that a distinct membrane is here present. Thus, if we treat the cells with water, they will endosmotically take up the latter, and become distended; but it is at the same time evident that a limiting membrane exercises a resistance against this distension, and, when the intercellular pressure becomes too strong, breaks. This endosmotic action makes it necessary, in examining the urinary canals, to use a solution of sugar, or some such concentrated fluid, instead of water.

This precaution is the more important, as precisely the largeness of the cells, and the consequently diminished calibre of the urinary canals are often the first indication of nutritive disturbance. On the other hand we may, by means of water, separate whole portions of urinary canals from epithelial cylinders; in the latter, namely, the cells will not be hindered from becoming distended outwards, and from so, instead of the smooth cylinder, producing an uneven one. In general, with the exception of those next the papillæ, the straight canals are of less calibre than the convoluted ones, but still their cavity is comparatively larger; this depends on the fact that the epithelial cells are flatter, while those in the convoluted canals are both larger in circumference, and also project more, protruding into the cavity of the canal. These last have, to a certain extent, more similarity to the epithelium of the glands; the first, to that of the excretory ducts of the latter.

The glomeruli are surrounded with a membrane with longitudinal nuclei, resembling the tunica propria of the urinary canals. The epithelium on the inside is not always easily perceptible; it has been supposed that it was continued over on the vascular loops themselves, but this is scarcely correct. The capsule has a tendency to separate; sometimes it is seen still adherent to the glomerulus, sometimes it is quite free, especially towards the edge of the section. Under the microscope the glomeruli, in general, present themselves in a rosette form, inasmuch as we do not see the whole course of the capillary loops, but only their convexity; out towards the edge, especially where the capsule is absent, the bulgings of the individual capillaries are seen, often containing small shining bodies (blood cells). Such bulgings may resemble cells; they call to mind the depressions on the capillaries in the pulmonary cavities, which also have been taken for epithelium. On the addition of acetic acid numerous nuclei appear, which for the most part belong to the capillaries; if the observer has been so fortunate, which is, however, very rarely the case, at the same time to get the vasa afferentia and efferentia, he will see their transverse nuclei. According to Beckmann's

investigations (*Virchow's Archiv*, Bd. 20), which agree with Key's, the proper glomerulus, and, therefore, the vascular loops, are, at least in children, not constantly in adults, surrounded by a slight membrane of connective tissue, studded with stellated or fusiform cells, which in part lie immediately on the surface, and partly press in between the several vascular loops.

The Interstitial Connective Tissue.—This is of great importance in pathological anatomy; in the ordinary manuals of histology but little attention is given to it, or the nature of the tissue is misunderstood. If we examine a thin section of the kidney under the microscope, the epithelium having been previously, so far as possible, brushed off, we shall see the several openings of the urinary canals bounded by a slight transparent line (the tunica propria). It is evident that the transverse section of the convoluted canals will present all kinds of different forms, but the straight canals also will be met with under very various circumstances, at one time more perpendicular, at another more parallel to their axes. Now, as even the thinnest section always has a certain relative thickness, and every divided canal has a superior and an inferior opening, which, most frequently, do not cover one another, a careful application of the microscope is necessary to avoid false conclusions. To illustrate this more precisely, let us for a moment suppose a urinary canal ascending obliquely into a section, to the lower opening of which canal the microscope is applied. If we now compare this with the nearest adjoining urinary canal, whose upper opening alone we possibly see, or at least use a sa means of comparison, the distance between the two canals will seem considerably greater than it really is, since the wall of the obliquely ascending canal will be taken as a distance, as the eye must assume that the two openings lie in the same plain. The image becomes still more deceptive from the fact that the nuclei of the capillaries, encircling the urinary canals, appear through the thin wall, and are mistaken for nuclei belonging to the connective tissue. If, on the contrary, we raise the tube of the microscope a little we shall see the two openings approach more and more, and the actual distance first becomes apparent when the superior opening of the oblique canal lies accurately in the focus. Simple as this circumstance is, and easy as it is to demonstrate it in a diagram, it is equally easy to overlook it in practice. The distance between the several urinary canals is, in fact, very small. If we examine the drawing in *Kölliker's Gewebelehre*, (Third Edition, p. 205,) it is almost certain that the great distances to be found in most places between the urinary canals depend on such an inclusion of the wall; and when it is in the same place also stated, that the nuclei lying between the canals all belong to connective tissue, this must depend only on an accidental inaccuracy. It is, on the whole, a fault, that almost all drawings in the histological manuals are more or less diagrammatic; they ought to be either purely so, or perfectly true to nature; anything intermediate is only confusing.

If a person commences his microscopic studies by seeking for the corresponding images in nature he will soon lose all pleasure and courage in his labour if he does not know how to estimate the meaning of such a drawing. The proper interstitial connective tissue has been first definitively demonstrated by Beer. It is found both in the cortical substance and in the pyramids, but in greatest quantity in the first named. It consists of a hyaline intercellular substance, lying around the urinary canals, between these and the capillaries, with interspersed stellated and fusiform elements. It is more abundant in children than in adults. These investigations of Beer have recently been confirmed by Smidt (de Renum Structurâ Questiones, Göttingen, 1861—*Henle und Pfeufer's Zeitschrift*). This interstitial tissue stands in the vessels in connexion with the external thin layer of connective tissue already described by Kölliker, which gives the kidney its smooth surface, by filling up the inequalities which the highly convoluted and bulging urinary canals would otherwise form. In order clearly to demonstrate this interstitial connective tissue it is necessary to inject the kidneys, and to brush away the epithelium; by this the nuclei in the capillaries, which are inseparable from the connective tissue, are removed; finally, if the preparation be treated with carmine, the elements of connective tissue will become more distinct. In employing carmine, weak solutions must be used—a couple of drops of an almost saturated solution of carmine in ammonia to the ounce of water. It is best previously to moisten the preparation with a little spirit of wine or dilute chromic acid, whereby the albumen is precipitated and the colouring is promoted; the nuclei are coloured, afterwards the entire cells, and for this several hours are required.

Of the Pathological Changes in the Kidneys, which are referable to the term Morbus Brightii.

In studying renal diseases, the object of our investigation is to establish the three following principal points:—1. What tissue or tissues are affected (vessels, parenchyma, or interstitial connective tissue); 2, whether only the cortical substance or the pyramids are attacked, or both together; and lastly, 3, whether the affection is partial or diffused. In the commencement a definite tissue can always be indicated as the starting point of the affection; later this is most frequently not possible; the several tissues are dependent on each other, so that they are often consecutively attacked; but, on the other hand, the several lobuli possess also independence of each other, and we have already seen that the vascular system of the cortical substance and that of the pyramids are to a certain extent independent of one another. It is especially chronic affections of the kidney which will be the subject of our consideration in an anatomico-pathological point of view. Formerly these were comprised under the name of *Morbus Brightii*,

but this is a very inaccurate denomination, and one which conveys but little information. The older investigators properly included under this term only what we now call parenchymatous nephritis, whose terminal stage is the characteristic and striking granular atrophy. Although this form is by far the most frequent, modern researches have revealed other pathological changes in the kidneys, which clinical physicians have not yet succeeded in definitely diagnosing from the parenchymatous nephritis; and it becomes, therefore, necessary to refer these also to the designation *Morbus Brightii*, so far as such a name shall be retained as a common denomination for these extremely different conditions. The essential symptoms are the albuminuria and the diminished secretion of urine, while the so-called fibrin cylinders have not the signification which Frerichs ascribes to them; they occur, at all events, only in the parenchymatous nephritis, and not even constantly in that. Here are three essentially different affections to be considered, each occupying its own tissue:—1. The amyloid degeneration of the kidney; 2, the parenchymatous; and 3, the interstitial nephritis. They may complicate one another; nay, all three may be present at once; sometimes one, sometimes another occurs first, but the parenchymatous is most frequently the primary affection.

The Amyloid Degeneration of the Kidney.—Unlike the liver, and especially the spleen, where the amyloid degeneration most frequently can, with perfect certainty, be established microscopically, and without having recourse to the reaction, the amyloid kidney is, in the rarest possible cases and that only in the highest degrees of degeneration, to be recognised in that way, and never with absolute certainty. The reason of this is, that in the first named organs, the proper parenchyma is chiefly affected, in the liver the hepatic cells, in the spleen either the follicles (sago-spleen) or the pulp (waxy-spleen), while the affection has a much greater extent than in the kidneys, where as a rule it is limited to the vessels, and does not spread to the proper parenchyma. It is, therefore, difficult to give definite non-microscopical anatomical diagnostic signs, particularly as the disease scarcely ever occurs alone, but is most frequently combined with a parenchymatous or interstitial nephritis, or, what is not unusual, with a result of this last, the interstitial fatty kidney. This latter combination is not unfrequently met with in individuals who have succumbed in the later stages of syphilis; but in what relation the two affections stand to each other is difficult to decide; probably they are collateral, dependent on the same etiological element, as they do not necessarily go together; for under similar circumstances, we at one time meet amyloid, at another interstitial fatty kidney. It is true it is not syphilis as such, but the dyscrasia which it, or the mercury, that is most frequently employed, produces, which is the cause; consequently it acts upon the kidneys in the same mode as chronic wasting diseases, especially chronic affections of the bones, tuberculosis,

&c. In the vast majority of cases the affection is met with in a great number of other organs, particularly the liver, the spleen, and the intestinal canal, and so produces death. As to the characters assigned to the amyloid kidney, much importance is not to be attributed to them. The Vienna School has indicated a solid consistence as something essential; but this is often not met with; also the lardaceous or waxy appearance—hence the name lardaceous or waxy kidney—but precisely in the kidney which exhibits such an appearance, we will often in vain seek amyloid, while it may be found in others which have only the appearance of being the seat of parenchymatous or interstitial changes. Yet it cannot be denied that in extreme degrees of degeneration we may even, with the microscope, acquire a suspicion, approximating to certainty, of the presence of amyloid. We then see the glomeruli stand sharply out as enlarged, shining, prominent bodies of a whitish-grey colour; the same appearance is presented by the vasa afferentia and other arteries accidentally lying in the surface of section. But the only certain and decisive sign is the reaction, which ought never to be omitted, when we have the least suspicion of the presence of amyloid. If we pour a small quantity of tincture of iodine on such a kidney, and rub it with the finger over the surface, we will see the glomeruli and the vessels assume a wine-red characteristic colour. The seat of the degeneration is the vessels alone, and the question is then, what vessels are attacked, in what order, and in what manner. It is chiefly the small vessels; first and foremost the glomeruli and vasa afferentia, next the vasa efferentia and the capillaries in the cortical substance, and finally, the arteriolæ rectæ are also attacked; it is rarely that the large vessels in the cortical substance and the main trunks of the arteriolæ rectæ degenerate. It is very long before any other tissue is attacked, and most frequently this does not occur; only in very rare cases does the affection extend to the tunicæ propriæ. The change consists in the deposition of a substance in the walls of the vessels, rendering them thickened, rigid, shining, and diminishing their calibre. This substance, the amyloid, penetrates the several elements, destroys their structure, so that the characteristic texture is changed, the whole becomes, in fact, a structureless, homogeneous mass. The glomeruli become pale, void of blood, shining, and this almost asbestos-like lustre is extremely characteristic; frequently in the very slightest degrees of the affection a single such shining loop of vessels is seen. By reason of the change in the glomeruli, and the thence resulting diminished supply of blood, the cortical substance also becomes anemic, while the hyperemia increases in the pyramids. Hemorrhages, too, may arise in consequence of the increased pressure on the rigid inelastic vascular walls, and we therefore not unfrequently find reddish or brownish streaks and spots, corresponding to the blood extravasated within or externally to the urinary canals. The colour is of different shades, according as

the blood is fresher or has undergone changes, especially in its pigment. The amyloid degeneration is always bilateral.

There is, however, another kind of change in the renal vessels, which is not due to amyloid degeneration, but which, in the same manner thickens the walls, and makes them less permeable, and therefore deserves to be mentioned here. It is a change corresponding to the so-called end-arteritis such as is so frequently met with in the larger vessels. We here also find two different results: arteriosclerosis, produced by the organization of the newly formed elements, and atheromatous degeneration by a fusion of the latter in fatty degeneration. The first is met with particularly in the larger vessels, but it may extend even to the glomeruli; the second is most frequent in the glomeruli and the smaller vessels. This change in the renal vessels does not always coincide with a similar affection of the larger vessels, especially the aorta, but it does so in the great majority of cases. It is only the proper fatty degeneration we shall consider more particularly in its different stages. In the commencement, an increase of the nuclei of the capillaries is seen; these divide, move farther from one another, and thereby elongate the several loops in the glomeruli without at the same time increasing their calibre; as the exciting cause of such a hyperplasia, cellular pathology has, as is well known, assumed an irritation; of what nature this is cannot be definitely stated, probably it proceeds from the blood. At this stage the process may be stationary. If, however, it advances farther, small, finely-divided fatty particles begin to show themselves around the nuclei, and this always indicates a destructive character; these fatty granules increase, the nuclei disappear, and in their place we find an aggregate of fatty molecules. Thus a whole glomerulus may degenerate; and this must, of course, produce the same effect as the amyloid degeneration—diminished permeability and transudation, with a tendency to rupture of the walls. Dissecting aneurism, such as we so frequently see in similar changes in the cerebral arteries, is here much rarer. Such a fatty degeneration of the glomeruli may acquire a microscopic similarity to amyloid, the glomeruli becoming enlarged and of a greyish white colour. The reaction and microscopic examination together will remove every doubt. Instead of the smooth, shining glomeruli are seen, by transmitted light, dark, irregularly formed, scattered or continuous fatty masses lying in the course of the vascular loops.

A few words more about the reaction so often spoken of; for its correct employment is of the greatest importance in answering the question whether amyloid is present or not. In many cases the affection is mistaken, because the test is inadequately applied, and for the same reason—for the observer is unwilling to throw the blame on himself—it has been attempted to deny to the reaction the importance it really possesses. With iodine the amyloid parts are coloured of a beautiful

wine-red, with a slightly violet tint; the colour has some resemblance to that of blood; and such an artery treated with iodine may appear as if it were filled with blood. We, in general, use iodine, dissolved in iodide of potassium—tincture of iodine evaporates too rapidly—but a dilute solution, often presenting only a slightly brownish colour, is sufficient; if the solution is too strong it attacks the whole tissue, rendering it strongly brownish and opaque; with weak solutions we are able to act almost exclusively on the amyloid parts; and the strongly wine-red colour then comes out exceedingly well marked, in contrast to the light iodine colour of the rest of the tissue. One or more drops of the solution of iodine should be applied to the preparation, over which a needle or delicate scalpel is to be passed, so as to cause the iodine to penetrate better. As is well known the iodine evaporates rapidly, it must therefore be repeatedly renewed, that already applied being removed with a dry brush or bibulous paper. When the iodine and sulphuric acid reaction has succeeded well it should not give any pure blue, or, least of all, a bluish-black colour—for then the sulphuric acid has had a destructive action on the tissue—but a beautiful violet, somewhat tending to a bluish colour. A successful preparation presents a very beautiful appearance of the structure of the kidney; we may succeed in seeing a larger or smaller portion of an interlobular artery with its afferent vessels, and the glomeruli appended to these of a splendid violet colour, so that we might suppose we had injected the kidney itself. Unfortunately the colour is more beautiful than durable; even if we take ever so much trouble to preserve it and carefully cement the preparation, in the course of a few days the colour will have disappeared in consequence of the evaporation of the iodine. The following is the process:—The preparation, which has been treated with iodine in the manner detailed above, is freed from the superfluous fluid and covered with a glass; to the edge of this a very small drop of concentrated sulphuric acid is applied, and the preparation is laid aside for some hours, or a whole day. These are the original directions given by Virchow, and to which he still adheres. However, we rarely obtain, in this mode, very beautiful preparations; in some places the acid has acted too strongly, in others too feebly. With some practice and a little dexterity we can, by gently passing a glass rod, moistened with sulphuric acid, over the preparation, without actually touching it, quickly produce very beautiful results. The chief difficulty is to get the sulphuric acid to act in a concentrated form, and in the smallest possible quantity. Still it must be observed that the preparation ought to be examined while fresh; if it be one or two days old a beautiful colour is never obtained—it becomes of a dirty greenish; on the other hand the reaction succeeds well in preparations which have been preserved in spirit or chromic acid.

The Parenchymatous Nephritis.

Although the relation of the epithelium of the renal canals to the

urinary secretion has not been satisfactorily explained, it is nevertheless certain that it plays a part; still the importance of the epithelial cells for the secretion is not uniformly great, which is due to the different structure they exhibit in different places. It has already been mentioned, in speaking of the anatomy of the urinary canals, that the epithelial cells in the convoluted canals, especially in those which lie next to the glomeruli, and therefore first come in contact with the secreted urine, are larger and richer in albuminates, and correspond most closely to the epithelium of glands, while those in the straight canals are flatter and poorer in contents, and resemble the epithelium in the excretory ducts of glands. Under pathological conditions, too, there is a difference in the importance of the epithelium; a disease which attacks the first-mentioned epithelial cells, and renders them inactive, produces a change in the urinary secretion, and thereby acts injuriously on the economy of the whole body, and becomes dangerous to life; if, on the contrary, the process is confined exclusively to the epithelium of the straight canals, only abnormal constituents are mixed with the urine, the secretion itself is not interfered with, and such an affection is consequently of much less importance. We have, therefore, under the parenchymatous nephritis, in the more extended sense of the term, two different processes to consider: the papillary catarrh, which attacks the epithelium in the straight canals, and the proper parenchymatous nephritis, which attacks the proper parenchyma, or that which is of importance to the secretion.

The Papillary Catarrh, or catarrhal nephritis, is tolerably frequent, although not so frequent as the bronchial catarrh, with which it may be compared. It is often continued from the bladder, or even from the urethra, though it may also derive its cause from within, as after the use of cantharides and acid diuretics. It is frequently complicated with parenchymatous nephritis; and possibly the affection, slight in itself, is then the starting point of the more important parenchymatous process, in the same manner as an ordinary bronchial catarrh, when long continued, may become the source of more serious affections, as tuberculous peribronchitis and broncho-pneumonia. In hard drinkers, in whom this combination of papillary catarrh and parenchymatous nephritis is often met with, this is perhaps the case, for alcoholic drinks also may, by continued use, give rise to chronic papillary catarrhs; yet, in these, the parenchymatous nephritis may also depend on the general disturbance of nutrition, from which such persons suffer. The seat of the affection is, as has been observed, the straight canals, and especially the papillæ. Here a whitish or slightly yellowish, somewhat shining striation is seen, corresponding to the course of the urinary canals; and, by the side of this, hyperemia is observed in the intervening vessels. On pressure, a more or less abundant quantity of a milky or creamy fluid

flows from the openings in the papillæ. In those cases where the disease is produced by cantharides or such like acrid matters, great hyperemia and ecchymosed spots are observed over the whole kidney, both on the surface and in the parenchyma itself. If the disease lasts longer, the distended urinary canals exercise a pressure on the vessels, the hyperemia diminishes more and more, and at last is seen only as a wreath set around the base of the pyramids. As to the peculiar nature of the disease, it is certain that in many cases it is limited to a production of cells and exudation of the latter, mixed with mucous catarrhal products; in these cases, Johnson's name of desquamative nephritis is very suitable; but the process cannot be compared to the desquamation of the epidermis, as in scarlatina, for this is the termination, the former the commencement of the affection. But it is, on the other hand, also indubitable, that a process similar to that which takes place in the proper parenchymatous nephritis may exist, with fatty metamorphosis and destruction of the whole epithelium. This occurs especially in the more acute, and, if we will, more degenerated cases, such as is not uncommonly observed in cholera, ichorous infectious diseases, and severe croupy pneumonia. Microscopic investigation will afford different results in consequence hereof. In the first case we shall find the dilated and bulging canals filled with a quantity of cells, with all the signs of an abundant production; in some we see a round, in others a more extended nucleus, as if on the point of dividing, and in others again we observe two or three nuclei. In two cases of papillary catarrh in children I saw at the same time ramified cells—the only case in which I have met with such; some of them closely resembled multipolar ganglionic cells, and the contents extended into the offshoots; others were more fusiform or club-shaped, running out into a long, delicate thread. Where several such cells lay together it gave the idea as if these different forms had arisen from lateral pressure caused by the abundant production of cells. The cells in the continued parts are in general very small, yet they never acquire the characteristic form and appearance of proper pus cells; in some pigment is seen, in others small fatty granules, just as in other catarrhs. Mucous products are deposited in the urinary canals also as soft, cylindrical, often plaited masses, with burst cells; they are incorrectly referred to the so-called fibrin cylinders. In the second case the urinary canals will also be distended, bulging, but filled with a detritus-like mass, formed of the wholly or only partially destroyed epithelial cells.

The Proper Parenchymatous Nephritis.—It was the so-called Bright's Disease which first led Virchow to propound his celebrated theory respecting parenchymatous inflammations. He showed that the disease is essentially connected with the epithelium, that it may be limited to the same, so that no free fibrinous exudation is deposited in the urinary canals; but that, on the contrary, the elements themselves are in a con-

dition to produce what was formerly mistaken for metamorphoses of a free exudation (inflammatory globules, fibrin-cylinders). He introduced the name parenchymatous nephritis. The disease, in general, attacks both kidneys. It is in this case the epithelium of the convoluted canals, therefore the proper parenchyma, which is affected. The changes the individual cells undergo are the same as in all parenchymatous inflammations. The cells begin by taking up a large quantity of solid nourishing materials (albuminates), whereby they become distended, larger (hypertrophied); by reason of the abundant, dense, finely granular protoplasm they become turbid, so that the nuclei are concealed; it becomes difficult to recognise the boundary of the several cells; they coalesce, or, as it were, adhere together; on the addition of dilute acetic acid, however, the nuclei again distinctly appear, and also, frequently, the membranes. In the next place small, fine, distinct, fatty granules begin to appear in the contents of the cells, and first around the nuclei; these increase more and more, while, at the same time, the nuclei disappear; the cells are at length completely filled with fatty granules, and finally vanish; the membrane, too, and the granular fatty mass become free (inflammatory globules). Easy as it is to follow the change in the individual cells, the changes thereby caused in the whole kidney become difficult to comprehend and to describe, and for this reason: that the process does not run a uniform course throughout the whole kidney; all the stages which the several cells pass through are often found united in one and the same kidney; finally, at a later stage of the disease, an interstitial nephritis most frequently supervenes. It is on account of these difficulties that a number of stages have been described, without rendering the state of things plainer or easier to understand. I shall describe only three stages, which are, of course, connected by intervening ones. The first stage cannot be anatomically distinguished from the condition which occurs in many acute diseases (typhus fever, scarlatina), and in many chronic affections, especially of the heart and lungs, or, in general, in all diseases with disturbances of nutrition. In infectious diseases (puerperal affections, glanders, &c.) there is found, simultaneously with parenchymatous inflammation in other organs, especially the liver, a similar condition of the kidneys. In the slighter degrees it occurs, therefore, so to speak, as a rule on dissection, and therefore it is exceedingly difficult to obtain perfectly healthy human kidneys for examination. This stage may be compared to the so-called stage of obstruction in croupy pneumonia. The kidney is swollen, œdematous, the capsule is easily torn off; on the smooth surface, which is less bright and shining than usual, there most frequently appears an elegant design of polygonal, or approximatively round figures, produced by the loaded interlobular vessels, which here and there form knotty points, as precursors of the subsequent stellated form. On cutting through the kidney, or on pressure, a bloody, serous fluid can be expressed.

On the surface of section hyperemia is observed in all vessels, sometimes also small extravasations of blood, in general in the urinary canals. The anatomical structure of the several lobules can often be observed in such kidneys much more distinctly than in perfectly healthy ones. The lobules are, that is to say, well marked out by the congested interlobular arteries, and these bear, on both sides, in rows, the red, point-like, striking glomeruli. The contours of the urinary canals appear well marked, especially those of the external convoluted ones, and of a whitish-grey colour. When the urinary canals present such an appearance it may even macroscopically be asserted that a change of the epithelium exists, for the tunicæ propriæ are too delicate and thin to admit of their being seen, under any circumstances, with the naked eye. The microscope confirms this assumption. The epithelial cells are seen enlarged, slightly opaque, projecting more than usual into the cavity of the canals; the glomeruli are congested, but are otherwise natural.

The second stage will, if the first be compared to the stage of obstruction in croupy pneumonia, correspond to that of red hepatization. The kidney is of normal size, or somewhat enlarged, but flaccid, indicating a change in the elasticity of the connective tissue. The surface is smooth, or presents a slightly velvety unevenness in consequence of the increased volume of the several lobules; the capsule separates easily. On section, the cortical substance appears somewhat broader and anemic, because the interlobular arteries have been emptied by the pressure of the urinary canals becoming more distended; the boundaries between the different lobules are not recognisable, nor is the contour of the several urinary canals; the whole cortical substance is of a dull white uniform lardaceous appearance; the glomeruli are anemic, small, hard to recognise; the form of the pyramids is preserved, their canals are either wholly free or are but slightly obstructed. Papillary catarrh is not unfrequently present. From the base of the pyramids a strongly reddish colour is observed, spreading over the whole pyramid, with the exception of the papilla, which is most frequently pale. This is, as nearly as possible, the Bright's kidney. The unpractised eye may find it difficult to distinguish such a kidney from an anemic but otherwise healthy kidney; but in the latter the surface of section is reflecting, the eye is able as it were to penetrate into its substance; in the former it is dull and opaque. On account of the difficulty of efflux of the venous blood a portion tends to the surface of the kidney, through the stellulæ Verheynii, to the capsule and adipose capsule, which are loaded with dark venous trunks. In the renal vein, too, the blood will flow more slowly, so that thrombi may form, and be continued thence into the vena cava—nay, completely up to the heart; detached particles may form infarctions in the lungs; usually, however, such thrombi become organized to form connective tissue, and are thus rendered less dangerous. In this stage an interstitial nephritis often

supervenes, and especially when the process runs a more chronic course; if its course is, on the contrary, more acute (acute Bright's Disease), the affection is confined to the epithelium. In puerperal fever the process is often very rapid, with uremic symptoms; likewise, in the parenchymatous nephritis accompanying the so-called yellow atrophy of the liver, to which the typhoid symptoms are referable. Under the microscope the urinary canals appear, in this stage, bulging, distended, occasionally varicose; the cells are hypertrophied, highly obstructed, some fatty granules begin to form; the epithelium of the straight canals is always more or less affected; occasionally the shining fibrin-cylinders are seen in these canals. Retrogression is, therefore, in this stage at least, possible. With it the acute active process terminates, and—

The third stage becomes, properly speaking, only a result of the morbid process—if we will a resolution, a recovery, although with loss of substance. This stage is the complete fatty metamorphosis of the cells—a process which, with respect to many new formations, we call a resolution; the difference lies not in the process itself, but in the different importance of the elements. Perhaps here, also, as well as in the proper resolution, a resorption takes place of the emulsive mass formed by the fusion of the cells; the greatest part is, it is true, removed with the urine; but where the glomeruli are destroyed this is not possible, and yet it is a fact that the urinary canals become empty and collapse.

The kidney may still, in this stage, in rare cases, be large and flaccid; and this indicates a relatively rapid course of the process; but in general it is small, shrunken, and firm to the touch. The capsule is difficult to separate—though this is not always the case; this adherence of the capsule may also be due to a perinephritis, especially to an inflammation of the layer of connective tissue enveloping the kidney; and in such cases portions of the renal parenchyma will usually come off when the capsule is separated. On the surface of the kidney numerous dark and congested venous trunks and stellæ are observed; it is uneven, in the extreme degrees of the affection, with greyish or yellowish elevations, some of them as large as peas; and between these finer granulations are seen, scattered here and there, calcified glomeruli, and not unfrequently small cysts; the substance is hard, almost schirrous to the knife. On section, the cortical substance is found to be greatly diminished, slender, often forming only a shell around the pyramids, and of the same appearance as the granulations. The process has also extended into the pyramids, and it appears, therefore, as if the cortical substance had pushed in and dislodged a portion of them; their base appears truncated, their feathery structure is lost; the glomeruli are small, anemic, and are, most frequently, not recognisable except when they are calcified. Frequently we find in such kidneys cysts, whose connexion with the urinary canals was first demonstrated by Beckmann; they vary from a microscopic minute-

ness to the size of nuts; they are filled with a gelatinous mass, and invested with an epithelium which is formed by the constriction, or complete cutting off of the urinary canals by the irregular contraction, and which is more and more distended by the accumulated mass of the destroyed cells. In such small microscopic cysts and constrictions fibrin-cylinders are usually found; most frequently in the pyramids, rarely in the convoluted canals. The granulations correspond to the still relatively sound, or rather least affected parts—the depressions between them to the empty collapsed canals; such collapsed places often present a reddish colour, the pressure on the vessels having ceased, and the circulation having thus again become freer. On account of these granulations the granular atrophy has been compared to the so-called cirrhosis of the liver, and has received the name of cirrhosis of the kidney; but it is only an external, superficial resemblance which exists between them, no essential agreement is to be found between the two processes. In the liver it is, as is well known, not the hepatic cells which are primarily attacked, but the inter-acinous connective tissue—while in the kidneys the proper point of issue lies in the parenchyma itself. The parenchymatous process is, in this stage, seldom pure, usually it is complicated with an interstitial nephritis; and on this the appearance of the surface will depend. The latter affection, too, is at length attended with granular atrophy, but in general with greater elevations. If both processes proceed equally and uniformly throughout the whole kidney, a smooth atrophy may be the result. Microscopic examination exhibits most of the canals distended, often two or three times wider than is normally the case—bulging; and this is also true with respect to the straight canals, with here and there the constrictions and cysts already mentioned. The contents are an emulsive mass of detritus, with fragments of cells of various sizes; some canals are seen collapsed and folded, and in others, again, the epithelium is still comparatively well preserved. The glomeruli are usually small, corrugated, surrounded by thickened capsules of connective tissue. The epithelium or the glomeruli themselves may be in a state of fatty degeneration; the latter are sometimes amyloid—some, as already mentioned, may have become calcareous.

The Interstitial Nephritis.

The change in the interstitial connective tissue may either preferentially affect the intercellular substance, which becomes thickened, hypertrophied, while the cells at the same time become larger, but only in a slight degree increased in number; or it may be the cells which the active irritation affects, multiplying by frequent subdivision, while the intercellular substance is not particularly increased. This last is, in general, a more acute process; in its extreme degree the cellular hyperplasia becomes rapid, the several elements as a result hereof becomes less and

less, and at length assume the characters of pus-cells. The process then becomes suppurative; in its lower degrees a pale cellular hypertrophy takes place, which may exist as such for a long time, and finally, as in the first case, pass over into induration (sclerosis). A third but rarer result is the interstitial fatty kidney, where the newly-formed connective tissue passes over into fatty degeneration. The acute suppurative nephritis has nothing to do with this. The first form and the lower degrees of the second may be classed together under the denomination of

The Simple Interstitial Nephritis.—Sometimes the cells, sometimes the intercellular substance, are predominatingly increased; this lesion is either partial or general; it attacks both kidneys, and runs a chronic course. The connective tissue formed around the urinary canals contracts, and at the same time the small cysts already mentioned are often formed. The same takes place around the glomeruli; the connective tissue here becomes denser and denser, and begins to contract cicatricially; it is at the same time compressed, the circulation is partially or wholly obstructed; finally, the loops may coalesce, and the glomerulus then at length forms only a homogeneous mass; beside this they may be amyloid or in a state of fatty degeneration. The elements lying in the structureless capsule constitute the starting point of these new formations of connective tissue, not as Frerichs and Rokitansky, nay, even Virchow at an earlier period (*Archiv*, Bd. 4) assumed, a fibrinous exudation. Gradually as the contraction increases the whole kidney becomes much diminished, the tissue becomes sclerotic, tough, anemic; the surface is uneven, with most frequently considerable elevations and depressions; hence there is granular atrophy, the elevated parts corresponding to those least affected. With a uniform new formation and contraction of the connective tissue, a smooth atrophy may also in rare cases arise. Most frequently, as has already been stated, a parenchymatous nephritis is at the same time met with; but in what relation the two affections stand to one another, which is the primary and which the secondary, it is difficult to decide. Under the microscope the interspaces between the urinary canals are seen to be increased, filled with a sometimes more hyaline, sometimes more nucleated substance. As a result hereof the urinary canals are slender, of small calibre, sometimes constricted so as to resemble a string of beads; the tunica propria is often thickened and streaked. The glomeruli are seen to be surrounded with thick, concentric layers of connective tissue; they are small, of a homogeneous appearance, or in a state of greater or less fatty degeneration.

In our day, when interstitial nephritis is, so to speak, in fashion, we must, especially in the slight degrees, be very cautious in diagnosing it; it is well known that what we look for is only too easily found. It has already been stated how the distance between the urinary canals may, to a careless investigator, appear to be increased without being really so; but there are also other interstitial changes, besides the interstitial nephritis,

which in reality may increase the distance. In diseanes of the heart, and in general in all cases where there is a possibility of venous stases, the interstitial venous plexus in the kidneys becomes thus distended with the stagnating blood, the vascular walls are thickened, and the urinary canals are, at the same time, also compressed; in other cases the capillary nuclei may, in the same mode as has been described with respect to the glomeruli, during the amyloid degeneration, be increased; they will indeed then, in general, lie tolerably regularly in rows; but as it is impossible to distinguish them from the nuclei of connective tissue, it will be evident that the diagnosis may be extremely difficult, unless recourse is had to the chemical reaction; the difficulty will be still further increased when to such processes an interstitial nephritis is actually superadded

Hitherto, in speaking of the interstitial nephritis, we have referred only to the cortical substance; but the case is quite different in the pyramids, and when it becomes circumscribed (Nephrit. syphilit.). In the *pyramids* it occurs either more limited, as small greyish tubercles between the pyramids of Ferrein, consequently as a kind of small fibromas—or more universally along the urinary canals; the tissue contracts here also, as the cortical substance and the urinary canals; at the same time, much more frequently undergo cystoid degeneration. The result is a contraction of the pyramids, both in length and transversely, though mostly in the first direction. The pyramids may, at the same time, acquire some resemblance to what is found in hydronephrosis; but in this case the pressure has worked only superficially, it does not proceed from the tissue itself, and the contraction affects the longitudinal axis, while the transverse axis is elongated.

The *circumscribed* interstitial nephritis, or so-called syphilitic nephritis, occurs in several scattered points, and is usually of a purely interstitial nature. It corresponds completely to the processes occasionally observed in persons who, at one period of their life have suffered from tertiary syphilis, in the liver, lungs, and other organs. The affected parts contract, and finally only depressed cicatrices remain after old hemorrhagic infarctions of long standing, with which, probably, such a syphilitic kidney has often been confounded. The two affections are, however, distinguished from one another by the fact, that in infarctions are always found products of the metamorphosis of blood (pigment, crystals of hematoidin), while these are not met with in the interstitial nephritis. There still remains to be mentioned the rarer *interstitial fatty kidney*. In this case the kidney is in general large and flaccid; on the surface are observed yellowish or whitish striæ and marks, which, on section, exhibit themselves over the whole cortical substance; they form irregular figures, so that it may, even macroscopically, be inferred that the fact cannot be referred to the regular urinary canals, but that it lies externally to them. This form occurs pure, but extremely rarely, most

frequently it is combined with amyloid or parenchymatous nephritis. It is, as has been mentioned, also supposed to be connected with syphilis. Under the microscope the urinary canals are seen pushed from one another, of diminished calibre; in the interspaces larger or less irregular fatty masses are deposited. Likewise the glomeruli, and in general the walls of the several vascular loops, are in a state of fatty or amyloid degeneration. The process may be compared with the atheromatous degeneration of the vessels, for in this case also the hyperplastic formations undergo a fatty metamorphosis.

Albuminuria, and, at least in the more advanced stages of the disease, diminished secretion of the urine have been enumerated as essential symptoms of the chronic renal affections, treated of in the foregoing paper; it is, therefore, desirable to see how these symptoms can be made to harmonize with the pathological changes just described It is well known that albumen exhibits only a very slight tendency to exosmose; but that such may take place, under increased pressure, is a settled point. Botkin's experiments with eggs (*Virchow's Archiv*, Bd. 20) which had been deprived, by means of dilute hydrochloric acid, of their calcareous shells, gave the following results:—An egg treated in this manner, and placed in distilled water, had, at the end of four days, allowed albumen to exude; but if, on the contrary, it were laid in a concentrated solution of chloride of sodium, no albumen was demonstrated in the solution, unless the egg, before having been laid aside, had begun to putrify. In the first case a portion of water presses endosmotically through the membrane, and, in consequence of the increased internal pressure thus produced, albumen again exudes. In the concentrated solution no such endosmose takes place, and, therefore, no exosmose; where putrefaction is commencing, on the other hand, the internal pressure is increased, and the albumen exudes. It has been said that the pressure to which the blood is exposed in the glomeruli must almost necessarily produce an exudation of albumen. This pressure is, in fact, exceedingly great; all the blood in the five or six vascular loops, of which the glomeruli are composed, is conveyed at the same time to the narrow exit, the vas efferens, which is not much wider than each single one of these loops. The pressure would be much greater still if the case were really such as is represented in most manuals, that the vas efferens is given off at an acute angle; but we have seen that this is not the case, and that, on the contrary, it is given off at an obtuse angle, and most frequently forms a curve, whereby the pressure produced by the interruption to the current of blood is somewhat diminished. Important and interesting as the question is, respecting the condition of the urine in the glomeruli, not only with reference to albumen, but also with respect to its reaction, whether this be alkaline, which is, perhaps, most probable, or acid, from

the beginning, it is scarcely possible to arrive at any certainty upon the point. Although this assumption respecting the physiological separation of albumen in the glomeruli is purely theoretical, there is still something in its favour. As the normal urine does not, in fact, contain albumen, the latter must again disappear in the urinary canals, and we are, therefore, naturally led to seek the intervening agent in the epithelial cells; if these are, however, morbid, and not in a condition to execute their depurating part, albuminuria is a necessary result. In the physiological state, too, we may find an argument in favour of this view, namely, in the structure of the epithelium already spoken of; the latter is in the section of the urinary canals, lying next to the glomeruli, rich in albuminates, which gradually diminish and almost disappear in the tubuli recti.[a] Against the occurrence of this separation in the glomeruli, on the other hand, the pathological fact is decisive, that in the extreme degrees of nephritis and amyloid degeneration, where the glomeruli are all but destroyed, and, at all events, cannot be supposed to allow the albumen to transude through their walls, great albuminuria still exists. We are consequently led to refer this transudation of albumen to another section of the vascular system, namely, the interstitial capillary net-work, that is, we turn from the more secreting to the more nourishing section. That it is in this case the increased lateral pressure which brings forth the albumen, is shown also by Frerichs' experiment of tying the aorta below the renal artery. If, that is to say, the glomeruli were the place where the albumen is separated, we should, since the pressure must here first affect the glomeruli, have a considerable albuminuria—but this is not the case; it appears, on the contrary, that the latter occurs most abundantly when the efflux of the blood is arrested, as when the renal vein is tied, that is, where the increased lateral pressure particularly affects the interstitial venous net-work. Yet we cannot, therefore, say that the albumen is excreted solely in this place; it is still possible that it may also, in some cases at least, be eliminated in the glomeruli; indeed it is even possible that the large epithelial cells abounding in albumen during the nutritive change, which suffer in the parenchymatous nephritis, might eliminate albumen. Still, disturbances in the circulation remain to be considered as the most essential source of albuminuria, and closely connected with it, whether the cause of these is to be sought in the vessels immediately belonging

[a] Should this theory prove to be well founded, it would go to explain, and would also receive support from, the fact observed by myself, of the very frequent, if not invariable occurrence of albumen in the urine of the human fetus. Of thirteen specimens of fetal urine, which I have on different occasions examined, all were more or less albuminous. See experiments in Dr. M'Clintock's paper, "On the Presence of Urea in the Liquor Amnii," in Vol. vii. of the present series of this Journal, p. 39; and also "Experiments as to the Existence of Sugar in the Urine of the Fetus," by W. D. Moore, M.B., in Vol. xx. of the same, page 88.—*Translator.*

to the kidney, or in more general disturbances, as cardiac and pulmonary diseases. We may, therefore, also have passing albuminuria of a purely transitory nature, where a mechanical pressure acts for a time on the large vessels (the aorta or vena cava), as we often see during pregnancy. In the chronic renal affections, treated of in this paper, we have also seen the venous stases to be a constant symptom; only in the amyloid degeneration, so far as it is not complicated with any more considerable parenchymatous or interstitial nephritis, is the case somewhat different. Here the cortical substance becomes anemic, on account of the diminished supply of blood, through the degenerated glomeruli, but at the same time the interstitial capillary net-work is, precisely for this reason, under an increased pressure, and the albuminuria may, therefore, be supposed to proceed from this source. The diminution in the secretion of urine, of course, depends upon the state of the glomeruli. Even if these are still acting, their calibre may be so diminished that only a small quantity of blood can traverse them, and their walls may be so changed that only a smaller quantity of material than ought to do so is in a condition to pass through them, and it is precisely such matters as ought to be excreted, and which constitute the most essential constituents of the urine (urea, uric acid) that are retained; hence a diminished, and at the same time, qualitatively altered, urine will be secreted. In other cases, where the glomeruli are not altered, or, at all events, are inconsiderably so, the urine will not be qualitatively, or even quantitatively altered, but will only carry off the abnormal albumen.

Finally, in conclusion, some remarks upon the so-called fibrin-cylinders, which were formerly considered to be a sign of inflammation, as a fibrinous exudation from the hyperemic vessels, whence the name derived from a time when all kinds of albuminates were called fibrin on account of the similarity of their reaction; the decisive character of fibrin is the fibrillar form in which it coagulates. Were they really an inflammatory product they should always be met with, and precisely in those places where the inflammation is most strongly marked; but this is not the case. It has already been casually mentioned that they are found principally in the straight tubes in the pyramids, more rarely in those of the cortical substance, and scarcely at all in its proper convoluted tubes, and that they are often situated in the constrictions and in the small microscopic cysts. Their origin is very obscure; according to Key, their formation depends on a change in the albumen of the epithelial cells. Virchow, too, inclines to this view. They scarcely even consist of fibrin, although Beckmann and Virchow sometimes saw a fibrillar structure; they are most nearly analogous to the so-called colloid mass. They appear at one time quite pale, shining, transparent, at another slightly inclining to yellowish; in other cases they enclose the discolourized blood cells, pigment, finely divided fat, epithelial nuclei, and other remains of epithelium;

they often pass away with the urine, when they do not lie in too strongly constricted places; if they lie in the small cysts, they gradually dissolve in the contents of the latter. Although the fibrin cylinders occur frequently in the parenchymatous nephritis, too great or too decisively diagnostic importance ought not to be attached to them. The cylinders occurring in papillary catarrhs are formed of mucin, and are purely catarrhal secretions.

CLINICAL RECORDS.

[It is intended to set apart a portion of the Journal, in this place, under the title of "Clinical Records," for reports of cases, together with such clinical observations as may be elicited by them; merely elementary matter being avoided, and lengthened disquisitions reserved for the first part of the Journal. It is hoped these communications will serve to make known the peculiar features presented by cases of frequent occurrence, which are now lost; to further the acquisition of a knowledge of the natural history of disease; to render permanent, and widely diffuse, the valuable observations constantly falling from our Hospital Physicians and Surgeons; and, at the same time, direct attention to special points, and afford materials for more elaborate essays.—Ed.]

Case of Melanosis. By Maurice H. Collis, F.R.C.S.I., M.B., &c., Surgeon to the Meath Hospital and County Dublin Infirmary, Member of Council R.C.S.I.

There are, at least, two distinct forms of melanosis of the eyeball:—1, that in which the melanotic matter is simply a secretion in excess, of pigmentary matter; and 2, that in which it is conjoined with cancerous deposits.

Both forms of disease are capable of permanent cure by operation, but in widely different ratios. Both are also liable to produce secondary deposits throughout the body generally, but the character and situation of these deposits are very different.

The case I now wish to place on record is one of the first class, or simple melanosis, in which removal of the eyeball was followed, not by local relapse, but by a slow deposit of melanotic matter in the sebaceous follicles of the skin, one tumour appearing after another until the entire surface of the body was covered with tumours of various sizes.

The patient, Mary Anne Heaffy, born in 1800, was sent to me by my friend, Dr. Bigger, in September, 1857, with a note to the effect that she had lost the sight of her right eye by melanosis. I admitted her into the Meath Hospital, September 21st.

She told me that sometime in 1855 she felt a sudden and severe dart of pain in the inner angle of the right eye. This pain occurred irregularly, about once each day; it gradually increased in frequency and duration, and extended to the outer angle, the ball of the eye, and the temple successively; it was accompanied by great watering of the eye and nostril. In spite of these pains she did not perceive that her vision had been impaired until nine months had elapsed. She then suddenly discovered that, with the exception of a faint glimmer of light in the lower part of the eye, all powers of sight were gone. This state of things slowly progressed for the worse, and on admission I found the following appearances:—There was much swelling of the upper lid, and a good deal of chemosis round the cornea The eyeball was distorted, slightly enlarged, and projected forward at the lower and inner quarter. The sclerotic at this point was thinned and prominent, and of a blackish hue. The cornea remained transparent, allowing us to see that the capsule of the lens was opaque, and that the lens, in its capsule, was pushed forward into the anterior chamber. The iris was not to be made out; it was, I suppose, drawn out of the way, leaving the pupil widely dilated and capable of permitting the displacement of the lens. The eyeball was perfectly natural in all its muscular movements.

For the sake of relieving a feeling of intolerable tension and pressure, I punctured the chemosis in several places, with great relief to the patient. On the next day I found that the sclerotic had given way at the prominent point, and that a shreddy black substance was oozing through the opening. This continued from day to day, and seemed to relieve her to some extent. Examination of it, by the microscope, revealed only granular pigment-cells, with no signs of cancer-cell.

The sight of the eye was quite gone; the patient was in almost constant pain and mental uneasiness; the disease was still quite local in its manifestation, and confined within the globe of the eye; the patient's general health was quite unaffected as yet. Taking these facts into consideration, and also that at any moment an extension of the disease might arise in the muscles of the orbit, or even in more distant parts, I held a consultation on the case, and with the concurrence of my colleagues, I removed the eyeball on the 5th October, 1857. The muscles were

removed along with the globe for the sake of ensuring the complete extirpation of all the diseased structures.

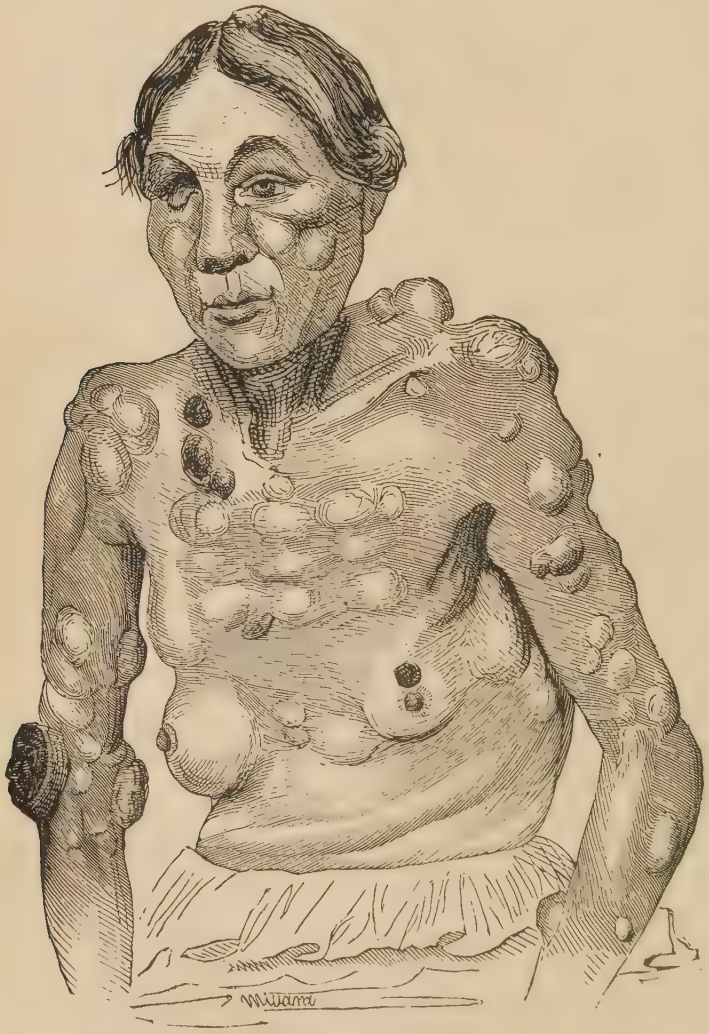
The tumour, when divided, was found firmer than the rest of the eye, and somewhat circumscribed; it consisted of amorphous colouring matter, oil globules, and aggregations of coloured globules. No cancer-cells were to be found in any part of it; nor did it resemble cancerous growths in any particular of outline or consistence. The colouring matter had spread into the vitreous humour, retina, capsule of the lens, and iris; none was found in the lens or sclerotic (even where thinned by pressure of the tumour); nor was there any in the optic nerve or muscles. The lens was still in its capsule, but was pushed forward so as to fill the anterior chamber.

The case progressed favourably as regards the operation; the eyelids drew together and closed over the empty socket, and no return of the disease took place in the neighbourhood of its primary development.

I saw her several times in the course of the next year. In February, 1859, she came to show me a small globular tumour which had rapidly formed under the skin of the right breast, near the axilla, with some radiating adhesions towards the gland and skin. This had grown, with much pain, to the size of a bean, and had then ceased to grow larger. It was sore to the touch, of a hard tense feel, and showed the dark colour of melanotic deposit through the skin. I applied some lotio plumbi subacetatis to it, and, at the end of ten days, it had lessened a little, had got less painful, and bore handling without discomfort to the poor woman. I would have removed it, as it was then solitary, but she would not consent. I saw her again in 1860; she had then three distinct tumours in the right breast, quite isolated from each other, one in the left breast, and several on the arms, legs, and back.

The history of all was alike: a rapid painful growth to a certain size (from that of a nut to that of a chesnut); a complete cessation of pain, and a diminution in the dimensions of each tumour to a certain extent, with the almost simultaneous development of a new tumour elsewhere. Nothing could well be done for the relief of the poor woman, or to check the appearance of these tumours. I gave her various tonics to improve her general health, as it had suffered to some extent. In the summer of 1862 I got Mr. Connolly to execute drawings of the state of the body. On the left malar bone may be observed a group of irregular oval tumours; these are movable on the periosteum. On the neck, and all over the thorax and arms, they are thickly scattered, of all sizes. Most are of a dusky hue, as if the intense black was veiled by a layer of skin stretched over them. One on the back, between the shoulders, is of a glossy polished black, like the colour of plumbago. A large one on the loins, the surface of which is cracked, and bleeds considerably, is also intensely black. The legs and arms are more sparingly covered with

these growths, but some of them are as large as oranges. The majority are not larger than walnuts, or pigeons' eggs. About one-fifth are larger, and, perhaps, as many are no larger, than filberts. Numerous as the



tumours were then, they are, at the present date, much more numerous. The woodcuts (by Mrs. Millard) are accurate copies from Mr. Connolly's most faithful drawings. To the state of things thus delineated may now be added great numbers of fresh tumours—several on the face, of large

size, have greatly added to the distressing appearance of the poor creature, as well as to her sufferings. She is now confined to bed, wasted and

worn out by pain and discharge of blood from three of the largest tumours, over which the skin has given away. Almost daily, for the last year, she has been losing more or less blood from the three large tumours on the back, the arm, and the thigh; yet still she holds on to life with an unexampled tenacity. Her body is wasted considerably, but by no means to the extreme degree that would be supposed likely from the amount of the abnormal deposit. Until loss of blood had occurred to a great extent, her general health and appearance did not suffer. The tumours had multiplied to a great extent, and the melanotic deposit was

much greater than the cancerous deposit in the most extreme cases of genuine cancer, while the woman's general health was not impaired. In genuine cancer a quarter of the deposit would have been accompanied by a prostration of vital power, and an appearance of cachexia, such as in this case scarcely yet exists after steady hemorrhage for 12 months. The original constitution of the disease, as a local development of simple melanotic matter, its reproduction in the form of encysted tumours (having their seat to all appearance in sebaceous follicles), the absence of cancerous cachexia, properly so called, and the absence of all signs or symptoms of internal deposits, all lead me to conclude that this is a case of simple non-cancerous melanosis. The hemorrhage is a result of ulceration of the over-distended integument. I have seen exactly similar ulceration and hemorrhage from fibrinous, adenoid, and even fatty tumours, which, by their large size and peculiar circumstances of situation, or exposure to external injuries by friction, had over distended and burst through the skin. It is now six years since the first appearance of the primary growth in the eye, and four since that in the breast was first brought under my notice. A much smaller amount of encephaloid disease would long since have killed the patient; and had the melanotic deposit been complicated with encephaloid its destructive progress would have been still more rapid. Internal organs and bones would have been poisoned, and general cachexia would, undoubtedly, have appeared long before this to close the sad scene of suffering. The poor patient may yet linger some months, if I can judge from the slight change for the worse which has taken place in her between my last two visits.

Applications of the solution of perchloride of iron check any severe hemorrhage, and dressing with dry cotton wool keeps down the daily weeping of blood within moderate limits.

Another point suggested by this case is the question, whether, in a similar case, the eye should be removed. A priori, the course adopted seems the best; it seems to afford the fairest chance for the permanent removal of the disease. Still, it is just possible that if the eye had been left, a constant daily discharge of the melanotic matter from it might have been a vent or safety valve for the system. In any future case, at all resembling this, it might be well to bear this point in mind, and, if it seemed advisable, to try the effect of non-removal, at least for a time.

Poisoning with Belladonna Successfully Treated with Opium. By R. Macnamara, M.R.I.A., Professor of Materia Medica and Member of Council R.C.S.I., one of the Surgeons to the Meath Hospital, &c., &c.

The following case, in which poisoning by belladonna was successfully

treated by the internal exhibition of opium, is recorded here for the purpose of supplementing those most important and interesting cases detailed by my friend and colleague, Mr. Wharton, in the pages of the *Dublin Medical Press* for September 3rd, 1862; to this most instructive article I must refer such of my readers as seek to be made masters of the literature of the subject, contenting myself here with briefly condensing, for their instruction, a *résumé* of what he has, in the place indicated, so ably put together for the benefit of his professional brethren.

In the *Dublin Journal of Medical Science* for July, 1838, Dr. Graves drew attention to the value of belladonna as a sedative in cases of fever where the pupils are contracted. Dr. Joseph Anderson drew attention to the antagonistic action of opium and belladonna, whilst Dr. Lee was the first to subject these views to the *experimentum crucis* in a case of poisoning by belladonna, which he treated with opium; and in a case of poisoning by opium, which he treated with belladonna; abroad Dr. Lee speedily found imitators; but, so far as I know, I believe it was reserved for my friend, Mr. Wharton, to be the successful pioneer, in this city, of this new plan of treatment. I shall not pursue this subject further, but at once proceed to place on record the following case which was reported for me by my apprentice, Mr. Percival, the intelligent and attentive resident pupil in the Meath Hospital.

John Mulligan, aged two years and two months, was admitted into the Meath Hospital, under the care of Surgeon Macnamara, on the 31st day of October, 1862, labouring under the effects of poisoning by belladonna.

History.—His mother stated that she lives at Rose Hall, Templeogue, County Dublin, and that, about half-past nine o'clock this morning, the child got into an empty room in which there was a small pot containing extract of belladonna, of which the child must have taken some, for she found his face and clothes smeared over with it. Some time after, she saw the child fall, and then remarked that he had a wild look about the eyes, upon which she brought him into hospital at half-past three o'clock, p.m.

Symptoms.—On examination I found the pulse strong, the pupils greatly dilated, he picked and pulled at his clothes, and was delirious. The mother having brought the pot which was found with the child, the contents were at once recognised to be extract of belladonna.

Treatment.—On the patient being put to bed I gave an emetic consisting of sulph. of zinc. gr. x., and pulv. ipec. gr. vi., which had the desired effect; the matter vomited had no trace of belladonna. Surgeon Macnamara having been sent for, ordered an enema of castor oil and turpentine to be given, which came away without any signs of belladonna being present in it. A short time after, five drops of tincture of opium were given; after one hour three drops, and every hour after, two drops, until

the patient fell asleep, which was at a quarter before one o'clock, a.m., up to which time there was no apparent contraction of the pupils. He slept quietly until ten minutes before two o'clock, when he started up and began crying. I then remarked, for the first time, that the pupils had contracted a little; he fell asleep again in about five minutes, and slept quietly until half-past six o'clock, a.m., when all the delirium had passed away, and the pupils were a little more contracted. He has been running about the ward all day.

Result.—November 1st, at 12 o'clock, Surgeon Macnamara ordered two drops of the tincture of opium to be given, and he has been going on remarkably well ever since; six o'clock, p.m., going on well, contraction of the pupils apparent; ten o'clock, p.m., sleeping soundly.

November 2nd, three o'clock, a.m., slept soundly all night, the pupils still a little dilated. Ten o'clock, a.m., the patient has left the hospital cured.

Poisoning by Laburnum Seeds.—By John Popham, A.M., M.B., L.K. & Q.C.P., Physician to the Cork North Infirmary.

On the evening of the 11th August, 1862, a number of children, about ten altogether, were brought to the infirmary, suffering in various degrees from the effects of eating some leguminous seeds, which were described by them as growing in pods on trees, and of an exceedingly bitter taste. It was conjectured at the time, and verified afterwards, that the seeds were those of the cytisus laburnum, or common laburnum, whose legumes the children mistook for those of the vetch. Most of the children, fortunately, had partaken of them in but small quantities, being deterred by their nauseous taste, and thus escaped with transient symptoms of gastric irritation; but one little fellow, named John Neil, about six years old, had swallowed the seeds greedily, and was soon afterwards attacked with marked signs of acro-narcotic poisoning. He first complained of giddiness and pain of the head, and a sensation of dryness, heat, and constriction in the throat; these symptoms were speedily followed by an excruciating sensation in the stomach, from which he sought to get relief by rolling on the ground with his hands pressed to the epigastrium, screaming and kicking violently. After a time nausea and vomiting came on, and he threw up a quantity of dark brown grumous matter, very bitter; when brought to the hospital the pains of the stomach still continued, the surface of the body was cold, the face pallid and anxious, his movements were agitated, the pulse rapid and fluttering, breathing laboured; he had also a convulsive twitching of the muscles of the face, and the pupils

were widely dilated, and less sensible to light. Upon his admission Dr. O'Sullivan, house-surgeon, had heat applied to the stomach and extremities, and administered an emetic, followed by draughts of warm water. I found, on arriving shortly afterwards, that the emetic had produced free vomiting, and, as the abdomen was painful and tympanitic, I directed a turpentine enema which had an excellent effect. Sinapisms were also applied to the epigastric and cardiac regions, and to the spine; and the aromatic spirits of ammonia were given. But it was found very difficult to get him to take anything from the forcible struggles which he made, and also from some difficulty of swallowing; however, he drank some strong coffee, which roused him. After the pain had abated, which was about two hours from admission, narcotic symptoms came on, the breathing becoming slow and stertorous, so that it was necessary to shake him up roughly. When thus roused he was testy and impatient, answering snappishly and irrelevantly, and almost instantly relapsing into stupor. By unremitting attention in resisting those lapses into insensibility, and the steady administration of stimulants, external and internal, he began, at length, to show more consciousness, the animal heat rose, the pulse became firmer, and he sank into a quiet, though heavy, sleep, unbroken till a late hour of the morning, when he appeared quite recovered, except being weak and pale.

I find but little notice of poisoning by laburnum seeds in works on toxicology; in *Dr. Christison's Treatise* a brief account is given of their deleterious properties, less from his own observation than that of others, and leading him to class the laburnum seeds with the narcotico-acrids. He is inclined, however, to regard the narcotic as the preponderating quality. "In two cases," he says, "communicated to me by Dr. Traill, the effects seem to have been almost purely narcotic." The above case, however, while it justifies the opinion about the double action of the poison, places the symptoms of acridity in strong relief. There seems little reason to doubt that these seeds are violent local irritants, capable of causing death by gastric inflammation, were it not that the stupefying effects upon the brain and nervous system, both from the poison itself and the shock from overwhelming pain, tend to produce more speedy dissolution.

Clinical Reports of two cases of Lacerated Perineum Restored by Operation. By D. Lloyd Roberts, M.D., Surgeon to St. Mary's Hospital for Women and Children, Manchester, Fellow of the Obstetrical, and Corresponding Member of the Epidemiological Societies of London.

Case I.—*Perforation of the Perineum in a First Labour. Aperture nearly healed. Complete Laceration of the Perineum in the Second Labour. Operation.*

Cure.—Catharine C., aged 26, was admitted an in-patient of St. Mary's Hospital, on July 13th, 1861. She has had two pregnancies, both of which were favourably conducted to their full period. Menstruation commenced at 14, and continued, with regular periodicity, to the time of the first impregnation, although somewhat in excess of the normal quantity. Two years and a-half ago her first labour took place; it occupied 10 hours. The presentation was natural, but during the extrusion of the child, the head perforated the perineum, leaving a small portion of its anterior edge entire. The perforation extended posteriorly to within one-eighth of an inch of the anus, dividing only a few of the fibres of the sphincter ani. Through this abnormal aperture the child was born. Very considerable tumefaction and sloughing supervened; the labia being much swollen and inflamed. The inflammation was reduced by soothing treatment, and the cleft in the perineum afterwards gradually contracted, by means of the application of acetum lyttæ, iodine, and actual cautery, to the diameter of a No. 12 catheter. A very slight operation would now probably have sufficed to complete the cure; to this, however, she refused to submit, and left the hospital. Twelve months afterwards she was again in labour, the duration of which was four and a-half hours. As the head was passing the perineum, the latter, this time, was completely lacerated, the rent almost entering the anus. Inflammation and sloughing of the parts followed, and for seven weeks she was under the care of a surgeon, by whom lotions and poultices were applied. At the expiration of this time she entered the hospital in a very weak and anemic state, and with partial loss of power of the sphincter ani. Over the act of defecation, when effected naturally, and without the stimulus of purgatives, she still retained considerable control; but when aperients were exhibited—a measure often rendered necessary by the naturally constipated state of her bowels, dejections and flatus were often passed involuntarily.

A few weeks were occupied in preparing her for the operation for lacerated perineum; and on September 7th she was placed on the operating table. The margins of the perineum having been pared for half an inch in thickness, and about three inches in length, the edges were brought together and secured by three deep, and three superficial sutures of ordinary ligature; the parts were dressed and the patient placed in bed, on her side, with her legs bandaged together to keep the parts in apposition. The case progressed most favourably. The deep sutures were removed on the fifth day, and the edges were found united completely in its deep parts. The superficial portions that were not yet united, healed rapidly by granulation. The patient made a most perfect recovery.

Case II.—*Laceration of the Perineum. Procidentia Uteri. Operation. Cure.*—Mary Ann ——, aged 21. Menstruation regular. Subject to whites. She had a child two years ago. Her labour was not a very

difficult one; but five days afterwards, she got up and walked about. This was followed by a bearing down of the womb, which has ever since incapacitated her from continuing her work, owing to her inability to endure prolonged standing. Pessaries were recommended by one or two medical men whom she consulted. These, however, she was unable to wear.

State on Admission.—The uterus is completely procident and hypertrophied, with a sanious discharge oozing from the os. The lips of the uterus present the appearance of raw beef, and have a tendency to bleed on the slightest irritation. The os tincæ is very patent; a No. 12 catheter can be passed through it into the uterus to the extent of seven inches. The case, in fact, furnishes an excellent instance of elongated cervix so graphically described by Mons. Huguier. The procidentia admits of being reduced, but returns immediately on resumption of the erect posture. The constant dragging pain produced by the weight of the uterus has seriously undermined her health and spirits; the urine trickles involuntarily on the slightest exertion, or, sometimes, there is uncontrollable incontinence. The bowels are generally constipated, and require medicine to move them, The perineum was found to have been lacerated; the rent passing through some portion of the sphincter ani, but not entering the bowel. The countenance is pale and waxy, the appetite gone, and the pulse small and frequent. She has all the appearance of a woman suffering from an aggravated attack of uterine dyspepsia. After the general health had been improved by preliminary treatment, including rest, vaginal injections, and restorative medicines, I next directed my attention to the restoration of the perineum. The operation for lacerated perineum was accordingly performed. A piece of mucous membrane two and a half inches in extent, and shaped like a horse-shoe, having been removed from the lacerated portion of the perineum, the edges of the wound were brought together by means of the ordinary quilled suture, and the junction was still farther maintained by some superficial sutures. After the operation, the patient's knees were tied together with a bandage, the catheter was passed every four hours, night and day, and the bowels kept constipated by a daily dose of laudanum On the evening of the third day after the operation, the deep sutures were removed, and the wound was found to have united in its entire extent. On the seventh day the superficial sutures were also taken away, and, at the end of three weeks, the patient was allowed to leave her ward for the convalescent room. She was discharged perfectly cured.

I saw her a few weeks ago, when she informed me that she was quite well, and had returned to her work.

These two cases are instructive as well as interesting, and illustrate still further the benefits conferred by plastic surgery upon the female sex. Only a few year ago, these two miserable creatures would have been

told that their condition was irremediable; but, thanks to the exertions of Mr. Baker Brown in this country, and of M. Roux on the continent, the great advantages and frequent success of these operations are causing them to gain ground in general estimation, and the amount of hidden suffering that has been, or may be, relieved is not easily estimated. The first case is interesting, as presenting the somewhat unusual accident of perforation of the perineum, in the almost entire closure of the fistulous opening by suitable astringent dressings, in the presumed want of a perfect cure, only by the patient refusing to submit to a trifling operation; in the complete laceration of the perineum during the second labour, a consequence, no doubt, of the already existing fistulous opening. After recovery from her labour, she presented herself to me, and begged me to admit her into the hospital, and relieve her from her miserable state by operation, or any means I thought fit.

The second case is a more ordinary one. We may remark in it, first, the inefficiency of pessaries in keeping up the uterus, in cases where the perineum is utterly destroyed. Second, the long-neglected prolapsus producing ulceration and subsequent constitutional disturbance, resulting in a complete loss of health and spirits. Third, the therapeutic value of rest in uterine hypertrophy, a condition which few places besides an hospital can supply to women in her rank of life.

In both cases the interference with the union of the parts which would have been certain to arise from the act of defecation, was averted by a daily dose of tincture of opium, by which the bowels were controlled. After union has taken place, and when there is inclination to evacuate the bowels, I have found the administration of a simple soap enema very effectual in softening the hardened feces which the rectum is so likely to contain in consequence of the prolonged artificial constipation, and no ill effects have followed the plan.

The regular and periodical use of the catheter should not be lost sight of, as the irritation and tension of the parts, after the operation, are generally very great, and any urine trickling into the vagina, and getting between the edges of the wound, would be likely materially to interfere with the healing process.

If pregnancy should occur again to either of these women, there is little reason to doubt that rupture of the perineum may be avoided by due care and attention on the part of the accoucheur.

A Case of Diabetes Mellitus. By C. F. Moore, M.D., L.R.C.S.I., Medical Superintendent to the Middletown Hospital, County Armagh.

A. S., a spare woman, æt. 45, a labourer's wife, nine years married,

mother of several children, four now living, admitted to Middletown Infirmary, June 4th, 1861. Worn and hectic looking; fat much absorbed; great appetite and thirst; passes about 10 qts. of urine in the 24 hours, of a palish straw colour; mawkish sweet smell; sp. gr. 1040. *Drinks* 3 quarts buttermilk and water; water ad libitum; 1 quart sweet milk, and 1 quart broth, containing 10 oz. of meat.

The urine has a slightly acid action on litmus; turmeric gives no indication of change; a little boiled on a piece of flat glass, with 2 drops of liquor potassæ, became brown and gave distinct saccharine smell (burnt sugar).

June 7th.—Passes 10 qts. in $22\frac{1}{2}$ hours.

8th.—Passes $10\frac{1}{2}$ qts. in 24 hours; looks something stronger and fuller in flesh; the nurse has counted about 8 qts. of liquid ingested in last 24 hours. Ordered Dover's powder, 15 grs., in divided doses, in 24 hours, and to have a castor oil draught, as some colocynth pills, taken last night, have had no effect. Sp. gr. of the urine, passed at noon, 1037 at 60° Fahr.

9th.—Seems stronger; tongue clean; appetite great; omit the quassia mixture, which she has been taking the last few days; the oil draught to be repeated, as the first did not act. 12 qts. of urine were passed in last 24 hours, weather being very damp.

10th.—Little change in symptoms, &c.

11th.—State as yesterday. Diet is 2 lbs. white bread; 10 oz. meat (in 2 pints of broth); 1 egg; 1 qt. newmilk; buttermilk and water ad libitum. Weighed 7 st. 9 lbs. on 10th June.

13th.—Passed 9 qts. in 22 hours.

14th.—Pulse 96; very hot sultry weather for this season; ther. 75° at 3 p.m.; and, owing partly to the effect of the weather on the skin perhaps, the kidneys have not secreted nearly so much, she having passed only 6 qts. of urine in last 24 hours; the sp. gr. of the first of this morning's being $1034\frac{5}{10}$; colour perhaps a little deeper than before—yellower or more canary coloured; sp. gr. of urine passed at 9 p.m. yesterday was 1036. Continue Dover's powder, grs. 15, in 3 doses daily; also the castor oil draught.

15th.—Pulse about 100; urine 6 qts.; continue Dover's powder and good diet.

16th.—7 qts. of urine this day.

17th.—6 qts.; the bowels have at last yielded, and now diarrhea has occurred.

18th.—5 qts. urine; diarrhea continues.

19th.—$12\frac{1}{2}$ pts.; pulse 84; diarrhea; omit the Dover's powder.

20th.—Diarrhea less; to have chalk mixture and laudanum; 7 qts.

21st.—$6\frac{1}{2}$ qts.; bowels more regular.

22nd.—6 qts.; 5 stools; omit powder; pulse 72.

24th.—Weighs 7 st. 11 lb., *i. e.* 2 lb. more than 14 days since. Diet as stated in report of 11th June.

25th.—Has been again taking the quassia mixture, which she likes, and thinks it agrees with her; bowels now regular; no Dover's powder last night, as it seemed to cause the diarrhea before; 6 qts. of urine.

26th.—Pulse 80; 6 qts.; sp. gr. 1036, at temperature 63°; in better spirits, and feeling stronger; feels warmer now; not cold and chilly as when she came in; has heartburn after breakfast; bowels not moved for 48 hours; the diarrhea some days ago weakened her much; never menstruated since before Christmas. About seven weeks before Christmas was "poorly," and continued so for about four weeks "off and on;" always a red secretion; had leucorrhea often before in the "intervals." Six months ago she "saw as much water as she does now;" she must have been ill a year; she got a great wetting, walking some 14 miles, in November, 1859, in "chilly weather;" she had bad shoes; it was "a blowing cold day;" was very wet from her knees down; does not remember if "poorly" at the time; felt chilly when she got home; did not feel ill, unless a pain in her back after; however, had this pain before that time; began to feel "thirsty" from that out, and to have to rise at night to micturate; had no cough. To have small doses of sesquicarbonate of ammonia, with the quassia mixture, twice daily; also to have 5 grs. of compound rhubarb pill at bedtime.

28th.—Urine 6 qts.

29th.—Urine 6 qts.; some pain in back; looks better.

30th.—Urine 6 qts.; pulse 72.

July 1st.—Weighs 7 st. 8 lb.; has two half ounce doses of rennet; urine sp. gr. 1035.

3rd.—Pulse 75; weakish; pain up her back from loins up; left foot numb and cold; no perspiration; continue rennet.

4th.—Urine 7 qts.

5th.—Urine 6 qts.

6th.—Urine 6 qts.

8th.—Urine 6 qts. in 27 hours; weighs 7 st. 9½ lb., and less thirst since she began the rennet; begins bran bread to-day.

9th.—Urine 6 qts.

11th.—Urine 7 qts.; fresh rennet being out, she tried a little slightly salted and dried, but had to leave it off, as it caused thirst; not feeling well; the weather is very wet.

12th.—9 qts. of urine; weather cold and wet, and it, as usual, affects her unfavourably; to have an oil draught.

13th.—Urine 8 qts.; recommences quassia mixture.

15th.—Urine 6 qts.; constantly requires castor oil; weighs 7 st. 10½ lb.; is very tired of the bran bread; weather to-day close and damp; to try

home-made bread, with half the usual quantity of bran, and raised with a little soda and buttermilk; begins rennet again in a diluted form.

16th.—Castor oil.

17th.—Weather cold and wet.

18th.—Sleeps better; less thirst.

20th.—7 qts. of urine; rennet finished; recommence quassia mixture.

22nd.—Weighs 7 st. 10½ lb., same as this day week; she had rennet on three days and quassia mixture on two days; latter made her thirsty, she thinks; to take 5 grs. of compound rhubarb pill at dinner.

31st.—Passes 7 qts. of urine; had continued the quassia mixture; is at times very weak, and almost constantly requires aperients. She weighed, 29th July, 7 st. 11 lbs. She left the hospital on 3rd August, and returned home to her husband and children, in an adjoining county; and though confined to bed, and possessed of a great appetite, still lives. (January 1st, 1863).

From the above it will be seen that the case has lasted from November, 1859, to January, 1863, more than three years. That the exciting cause was apparently a severe wetting, during several hours, at a cold season, when fatigued, superadded probably to exhaustion from scanty means and child-bearing.

Hospital treatment was beneficial, as the quantity of urine fell from 10 and 12 qts. on admission, to 6 qts. and less, in the comparatively warm part (July) of a cold and wet season, and had only advanced again to 7 qts. in August, when the temperature began to fall, and when she was, to some extent, suffering from nostalgia, superadded to her other disease, and she had gained 2 lbs. in weight while in hospital.

When the rennet was good it seemed to allay the thirst better than anything else, and to keep down the quantity of renal secretion as well, if not better, than anything else. The quassia mixture sometimes seemed to cause thirst; if a case again comes under my observation I will "ring the changes" on lighter bitters more than I did in this case.

Constipation was a troublesome and constant concomitant in her case, no doubt from the diversion of fluids from the alimentary canal to the kidneys. The state of the air as to heat, cold, and moisture, sensibly and painfully affected her as to her disease. I found no encouragement in using ammonia; Dover's powder, too, was only a temporary good, and then again it so constantly caused diarrhea that I had to give it up.

The specific gravity of the urine was 1040 on admission, but came down to $1034\frac{5}{10}$, and 1036 under hospital treatment.

As to keeping the patient from drinking I looked on that as impossible; as I also did the attempt at keeping up a constant supply of fresh rennet, even if a cure could be expected from its constant use.—*January* 9, 1863.

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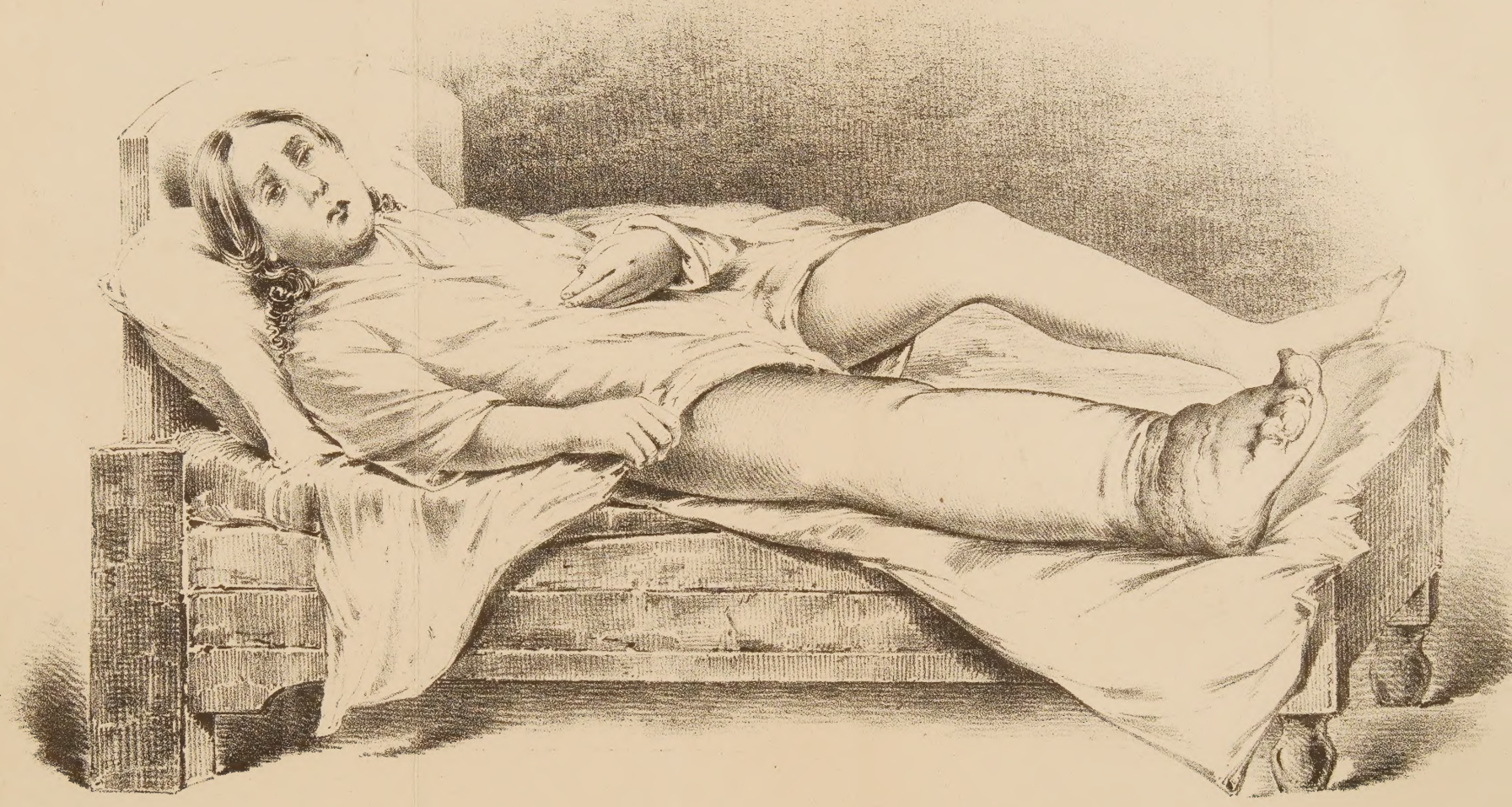

Mr BUTCHER ON THE TREATMENT OF ELEPHANTIASIS ARABUM
BY LIGATURE OF THE FEMORAL ARTERY.

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